

BINOCULAR RESOLUTION AND PERCEPTION OF RACE
IN SOUTH AFRICA*

BY THOMAS F. PETTIGREW AND GORDON W. ALLPORT

Harvard University

AND ERIC O. BARNETT

University of Natal

Our experiment uses a newly developed method for research in social perception: a brief stereoscopic presentation of pairs of racial photographs. Members of the four major racial groups in South Africa—Europeans, Coloureds, Indians, and Africans—serve both as judges and as photographic stimulus material. The factor of eye dominance is controlled.

While the stimulus material itself accounts for many of the findings, there is clear evidence that 'directed state' factors are also at work. White South Africans, especially the Afrikaners, seem to manifest a 'perceptual vigilance' in their judgements. They report racially mixed photographs to be either pure *European* or full-blooded *African* more often than do judges of other races. This tendency is consistent with the bifurcation (judgements of 'black' or 'white') known to be characteristic of people who are apprehensive concerning race relations.

The use of meaningful stimuli presented in a setting of binocular rivalry offers new promise for the study of cultural and personal determinants in perception.

I. INTRODUCTION

The role of meaningful content in binocular rivalry and fusion has not yet received careful and systematic attention. E. Engel (1956) and J. Bagby (1957) have each recently shown, however, the lively possibilities that lie in this unexplored avenue of research. Engel has demonstrated, for instance, that *right-side up* faces predominate over simultaneous images of faces *upside down*. Employing more complex stimuli, Bagby used both Mexican and American subjects. Such pictures as a bullfight and a baseball game presented simultaneously sometimes give meaningful fusions of the two, but more often result in a conflict. The culturally less familiar picture tends to be suppressed, the culturally more familiar image tends to be seen.

Precisely what variables are operating to determine the part meaning plays in binocular resolution cannot be specified as yet. Familiarity is clearly one important factor in the studies of Engel and Bagby. It seems reasonable to suppose that stereoscopic viewing of meaningful material may offer likewise a sensitive technique for studying a wide variety of 'directed state' problems in perception. (We are using F. H. Allport's (1955) term 'directed state' to cover all psychogenic and psychodynamic influences on perception.) H. Cantril (1957) has suggested, for example, applications of the technique to psychiatric research.

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The present paper uses the technique in a study of race relations. The general problem under investigation is to ascertain the principal variables related to identification of race (We use the term 'race' throughout this paper to distinguish the four major ethnic groups of South Africa without claim to any biological or anthropological precision.) More specifically, we are interested in the 'directed state' question: *Do members of various ethnic groups identify each other differently?* This question is of special interest and importance in a highly diverse, multiracial society such as the Union of South Africa where the present study is set.

II. METHOD

(i) Apparatus

The research employed an Engel stereoscope which allows precise light and distance control. This apparatus is described in detail by Engel (1956).

The stimulus material consisted of similarly posed, full-faced, bust photographs of both sexes and of all four major ethnic groups of South Africa. These four groups are African (full-blooded Bantu Coloured (mulattoes, frequently also possessing some Malayan ancestry, who form a legally and popularly recognized separate ethnic group), Indian (Hindu or Moslem), and European (white—either English or Afrikaans-speaking). This material was readily available from a local photographer who had on file a large number of passport photographs of all groups (about $1\frac{1}{2}$ in. square). These photographs are required by the South African government in connexion with their Population Registration Act. From this rich assortment we selected pictures of subjects between the ages of twenty-one and thirty years, these pictures being regarded as typical of each of the four groups by two white South African social scientists.

For our practice series, four duplicated bust photographs of one male from each of the four racial groups were employed. That is, identical pictures were shown to the two eyes. The experiment proper used forty bust pictures, twenty of each sex and ten of each race. Twenty same-sex pairs were formed so that each group would be paired with all four groups an equal number of times. In addition, these pairs were equated for 'pleasantness' of expression through the independent ratings of three judges. The equalizing of all pairs as to pose, age, sex, and pleasantness maximizes the likelihood of the fusion of binocular stimuli.

(ii) Procedure

Unlike Engel (1956), who placed no time limit on the exposure duration, we limited each exposure to approximately 2 sec. in order to minimize rivalry and encourage fusion. Our chief interest lay in prompt racial identification. Pilot work indicated that the 2-sec. exposure was an optimal time for this purpose as longer intervals tended to induce uncertainty of judgement and fluctuation of images.

Though the Engel stereoscope allows for differential illumination of the two images, we maintained constant illumination for both eyes, using the maximum allowed by the 7 watt lamps.

The subject sat comfortably at a convenient height before the stereoscope. He was told that he would see pictures of people, some of them would be European, some Coloured, some Indian, and some African. At the signal 'Ready', the subject took the viewing position and with both eyes looked at the brief exposure of the two images. Immediately thereafter he named the race he thought he had seen represented, and if he made qualifications or seemed uncertain of his judgement the fact was noted. Actually, under these conditions we found that nearly all subjects made immediate and unequivocal judgement. The importance of this fact should be underlined. Whether the subject suppresses one image or creates a fusion of the two cannot always be known, but he promptly gives *some* resolution to retinal conflict on the basis of *subjective* factors.

For the preliminary practice series there was no time limit, and since the two photographs in each pair were identical, fusion was excellent and errors of naming rare. Given in a constant order, these practice trials served four functions. First, the subject 'warmed up' and became generally accustomed to the experimental procedure. Secondly, the subject was able to vary the distance of the pictures from his eyes; once he achieved maximum fusion at the optimal distance for him, this distance was maintained throughout the remainder of his experimental session. Thirdly, since he saw only the image of a single person (even if it were blurred or doubled in the process of adjusting the distance) this fact reinforced his belief that only one face was being presented to him each time. The fourth purpose of the practice

trials was to demonstrate that all four groups of South Africa—European, Coloured, Indian, and African—were represented in the pictures.

Immediately after the practice series, the experiment proper began. The following list gives the randomized order used (E = European, C = Coloured, I = Indian, A = African, m = male, and f = female):

- | | | | |
|-------------|--------------|--------------|--------------|
| (1) $If-Af$ | (6) $Em-Cm$ | (11) $Am-Am$ | (16) $Em-Em$ |
| (2) $Ef-If$ | (7) $Ef-Af$ | (12) $Cm-Am$ | (17) $Em-Im$ |
| (3) $Af-Af$ | (8) $Ef-Ef$ | (13) $Cf-Cf$ | (18) $Em-Am$ |
| (4) $Ef-Cf$ | (9) $Cf-Af$ | (14) $Cm-Im$ | (19) $If-If$ |
| (5) $Cf-If$ | (10) $Im-Am$ | (15) $Cm-Cm$ | (20) $Im-Im$ |

The picture listed first in each pair was presented to the right eye while the other photograph was presented to the left eye. Upon finishing the exposure of these twenty pairs, the list was reversed in order to control for eye dominance. Thus, with the twentieth pair ($Im-Im$) the same Indian male would appear in the twentieth trial before the left eye and in the twenty-first trial before the right eye. The twenty-second trial employed the same pair as the nineteenth trial but with the pictures reversed as to eyes, and so on until the fortieth exposure.

At the close of each experimental session, the investigator determined each subject's educational background, occupation, and age.

(iii) *The subjects*

One-hundred and twenty-two subjects from the Durban area representing five different ethnic groups participated in the study. The data from fourteen additional subjects were discarded by a previously chosen criterion of extreme eye dominance. In all of these rejected cases, at least eight out of the twelve two-race pairs (nos. 1, 2, 4, 5, 6, 7, 9, 10, 12, 14, 17 and 18) were consistently responded to by the label of the race represented to one particular eye. Thus, a right-eye dominant person would report with the first pair an Indian when the Indian was shown the right eye on the first trial and then report an African when the African was shown the right eye on the fortieth trial. When this happened eight or more times with the same eye the subject was not included in the final sample. The remaining 122 subjects all possessed normal eyesight in other respects as well as showing no severe eye dominance.

Many of the African and Coloured sample members of lower economic status received a shilling for their services. Other subjects were not paid.

The distribution of subjects is as follows. European Afrikaans-speaking, $N=20$: European English-speaking, $N=22$: Coloureds, $N=20$: Indians, $N=20$: Africans, $N=40$. For an understanding of our results it is important to note that the European subjects comprise two sub-groups: Afrikaans-speaking and English-speaking.

The subjects were residents of Durban, excepting for the Coloureds who lived in a village south of the city. The average age for all groups was in the middle twenties. Slightly less than half of each ethnic group were women. The educational range for the most part covered medium and low amounts of schooling; very few had matriculated in college. The possible influence of the educational variable will be considered later. The Africans were servants excepting for a few student nurses, while the Indians and Coloureds for the most part held lower skilled jobs; the European subjects were in middle class occupations. In spite of our efforts to obtain a homogeneous socio-economic group of subjects, the South African social setting makes such equating impossible to achieve.

Obviously no claim can be made that these samples were randomly selected and therefore representative of the various ethnic groups of the Union of South Africa. L. Kish (1957) has noted that such clustered samples frequently yield misleading probability statements when simple random sample statistics are used. Hence probability statements made in this article should be viewed as tentative.

Social conditions in South Africa today make it impossible for a single experimenter of any race to collect data from the various groups. Accordingly, the procedure was standardized as much as possible and various investigators tested different groups. We are indebted to Messrs Mayisela and Zuma for collecting data from African subjects, to Mr Ramphal for Indian subjects, and to Messrs Mann and Dickie-Clark for Coloured subjects.

III. THE GUIDING HYPOTHESIS

The exploratory nature of the present study precluded the use of specific hypotheses. One general hypothesis did guide the work, however: *members of different ethnic groups will vary in their racial identifications of each other*. Put differently, identification of race

is in part a function of a person's own race. In order to examine this hypothesized relationship further, the study varied the type of stimulus, using one-race pairs of pictures and all possible mixtures of two race pairs of photographs.

The special interest of the study focuses upon the European groups—particularly the Afrikaans-speaking Europeans. The reason, of course, is that at the present time the white people in South Africa are living in a state of considerable apprehension and are endeavouring to maintain white supremacy at all costs. According to the 'directed state' theories, such emotional tension might lead to either 'perceptual vigilance' (in which case an excessive number of the threatening races might be perceived) or to 'perceptual defence' (in which case an unusually small number of the threatening races might be perceived). A related hypothesis would hold that Europeans being under stress would tend to show excessive bifurcation of judgements. That is, they would judge too many photographs to be either European or African (white or black) and too few as Indian or Coloured.

Our attention was drawn to these possibilities through a small pilot study involving well-educated Afrikaans-speaking and English-speaking South African whites. While both groups undoubtedly are under strain owing to the racial problems of South Africa, the Afrikaners gave quite different judgements from the English-speaking. For example, while looking at a two-race stimulus involving both European and Indian pictures, the Afrikaners uniformly reported *European* (apparently suppressing the Indian image) while the English reported *Indian* or *Coloured* frequently. This result we tentatively ascribed to the Afrikaner's greater racial apprehensiveness.

In short, 'directed state' possibilities with the various groups of subjects—and especially the Afrikaners—are given particular attention and emphasis in the present study.

IV. RESULTS

(i) *General overview*

Table 1 presents the entire raw data obtained in this investigation, from which all later analyses will be derived. For purposes of illustration, let us read the first line. It shows that all eighty judgements of the European-European stimulus combination made by the twenty Afrikaner subjects (four judgements per subject) were *European*. For the twenty-two English-speaking subjects, the European-European combination was judged

Table 1. *Racial identifications of five observer groups in response to ten stimulus combinations**

Binocular stimulus combinations (four of each)...	Afrikaners N = 20				English N = 22				Coloureds N = 20				Indians N = 20				Africans N = 20			
	E	C	I	A	E	C	I	A	E	C	I	A	E	C	I	A	E	C	I	A
1. European-European	80	—	—	—	82	—	6	—	52	15	13	—	71	1	8	—	108	14	—	—
2. Coloured-Coloured	3	47	20	10	—	69	18	1	2	64	14	—	4	57	19	—	5	104	—	—
3. Indian-Indian	—	9	65	6	2	14	72	—	4	21	55	—	—	10	70	—	5	36	1	—
4. African-African	—	3	—	77	—	3	2	83	—	—	1	79	—	2	—	78	1	7	—	—
5. European-Coloured	34	28	11	7	32	45	5	6	32	35	12	1	21	52	5	2	41	73	—	—
6. European-Indian	43	7	29	1	38	10	40	—	37	3	39	1	20	1	59	—	52	11	—	—
7. European-African	20	18	14	28	28	20	18	22	16	21	23	20	18	28	19	15	31	38	—	—
8. Coloured-Indian	4	19	52	5	—	37	47	4	1	18	61	—	2	26	52	—	12	47	1	—
9. Coloured-African	—	22	7	51	—	34	10	44	—	34	6	40	—	37	2	41	—	76	—	—
10. Indian-African	—	4	47	29	—	5	60	23	—	5	57	18	—	9	41	30	—	27	—	—
	184	157	245	214	182	237	278	183	144	216	281	159	136	223	275	166	255	433	5	—

* E, C, I, and A refer to the responses made by the subject: E = European, C = Coloured, I = Indian, and A = African.

eighty-two times as *European* and six times as *Indian*. Continuing across the first line, we note the Coloured subjects saw the European-European pair as being *European* fifty-two times, *Coloured* fifteen times, and *Indian* thirteen times; while the Indian subjects saw the pair as *European* seventy-one times, *Coloured* once, and *Indian* eight times. Completing the line, the stimulus pair was seen by the forty African subjects as *European* 108 times, as *Coloured* fourteen times, as *Indian* thirty-six times, and as *African* twice.

(ii) *Racial identifications*

Since in fact the series of photographs employed an equal number of European, Coloured, Indian, and African, we can first ask, Do the judgements fall equally—25% for each group? Table 2 presents the percentages bearing on this question. For the one-race pairs (stimulus combinations 1-4) there was a strong tendency for the judgements to vary from an equal usage, 25% for each group ($2 \times 4 \chi^2 = 23.50$; $p < 0.001$). For these stimuli, *European* identifications were disproportionately underrepresented and *Indian* identifications disproportionately overrepresented.

Table 2. *Percentage of responses assigned to each group by all subjects combined*

	Judgements			
	European	Coloured	Indian	African
One-race pairs (combinations 1-4)	21.5	24.4	29.1	25.0
Two-race pairs (combinations 5-10)	16.5	27.0	37.1	19.5
Total (combinations 1-10)	18.5	25.9	33.9	21.7

For the two-race pairs (stimulus combinations 5-10) the tendency to vary from an equal distribution of 25% was even more pronounced ($2 \times 4 \chi^2 = 300.62$; $p < 0.001$). Judgements of *Coloured* and *Indian* were more numerous than might be expected, judgements of *European* and *African* less so. This same trend holds for all ten stimulus combinations taken together ($2 \times 4 \chi^2 = 260.78$; $p < 0.001$).

Summing up, the four racial identifications were not employed with equal frequency. The tendency for *Coloured* and *Indian* judgements to be more common than *European* and *African* judgements was pronounced for the more ambiguous stimuli—the two-race pairs.

Table 3. *Percentage of judgements assigned to each group by each subject group*

Subjects	Judgements			
	European	Coloured	Indian	African
Afrikaners	23.0	19.6	30.6	26.8
English	20.7	26.9	31.6	20.8
Coloured	18.0	27.0	35.1	19.9
Indian	17.0	27.9	34.4	20.7
African	15.9	27.1	35.9	21.1

The differences noted in Table 2 in the differential use of the four race labels raises the further question concerning the five groups of subjects. Do all groups of subjects evidence the trend noted in Table 2 or are there differences between the groups in the frequency with which they use the various labels? The relevant data for this question are summarized in Table 3.

All groups save the Afrikaners reveal the same general trend shown in Table 2: the

more frequent use of *Coloured* and *Indian* labels than *European* and *African* labels. Our group of greatest interest, the Afrikaners, however, reports more *Europeans* and *Africans* and less *Coloureds* and *Indians* than any other subject group. Though much less than the Afrikaners ($4 \times 2 \chi^2 = 16.80$; $p < 0.001$), the English participants evidence some tendencies in the same direction. Only chance differences exist between the Coloured Indian, and African subjects, all of whom show a strong tendency to over-use *Coloured* and *Indian* labels.

A glance back at Table 1 will show the reader that these differences between Afrikaners and other subjects in the use of racial labels are quite general in nature and not limited to a few isolated stimulus combinations. With all ten combinations, Afrikaans-speaking subjects tended with minor exceptions to see more *Europeans* and *Africans* than did the other four subject groups.

(iii) *The accuracy of judgements*

How accurate were the judgements of race for the pairs of pictures involving only one race? The reader will recall that there were eight pairs of such pictures, one for each sex for all four races (combinations 1 through 4). Each of these pairs having been shown twice, there were sixteen judgements made by each subject to stimuli which realistically represented only one race. The accuracy of these judgements is summarized in Table 4.

Table 4. *Percentage of correct identifications of one-race pairs*

Subject groups	The one-race pairs					Total average
	N	European	Coloured	Indian	African	
Afrikaners	20	100.00	58.75	81.25	96.25	84.06
English	22	93.18	78.40	81.83	94.33	86.94
Coloureds	20	65.00	80.00	68.75	98.75	78.13
Indians	20	88.75	71.25	87.50	97.50	86.25
Africans	40	67.50	65.00	73.75	91.88	74.53
Total average		80.53	69.88	77.87	95.08	80.84

Three interesting questions can be asked concerning accuracy. First, which races were most accurately perceived? Table 4 shows that the African pairs were by far the easiest to identify correctly (African-European corrected $2 \times 2 \chi^2 = 46.92$; $p < 0.001$), while the Europeans and Indians were of average difficulty (European-Indian corrected $2 \times 2 \chi^2 = 0.89$; not significant). The Coloureds were the hardest to identify correctly (Indian-Coloured corrected $2 \times 2 \chi^2 = 10.14$; $p < 0.01$).

Secondly, were some groups of subjects more accurate in their judgements than others? Immediately we notice that, except for the Africans, each race recognizes itself better than do others. Thus, Afrikaners and English are the most correct judges of the European pairs, Coloureds of the Coloured pairs, and Indians of the Indian pairs. Even relatively inaccurate Africans were at their best in judging the African pairs. We note in Table 4 sharp differences between the relatively inaccurate Coloureds and Africans on the one hand and the relatively accurate Afrikaners, English, and Indians on the other. The reader will remember, though, that the Africans and Coloureds were comparatively the poorest educated groups of our five samples. If education is an important factor in correct identifications, then Africans and Coloureds would have been expected to perform less accurately for educational reasons alone. Thus, we shall need to check later on the effect of education in this relationship.

A third question concerns the 19% incorrect identifications of the one-race pairs. How were these errors distributed among the four labels? Table 5 presents the pertinent data.

We note from Table 5 that when errors occurred in response to the one-race pairs they predominantly consisted of mistakenly labelling some race as *Coloured* or *Indian*. The two most accurately perceived races, African and European (see Table 4), were seldom guessed incorrectly. Only one exception to this trend occurs—the Afrikaans-speaking subjects again. Of their fifty-one errors to the same race pairs, the Afrikaners reported *African* far more often than all other groups combined and guessed *Coloured* and *Indian* less than any other subject group. A further check on our master chart, Table 1, reveals that these differences are largely a result of the Afrikaners' differential responding to stimulus combinations 2 and 3—the Indian and Coloured one-race pairs. These unpopular groups are seen as 'black'. Sixteen of the twenty-two times these pairs were mistakenly seen as African occurred with Afrikaans-speaking perceivers. This is a special case of the tendency noted earlier for the Afrikaners to see less Coloureds and Indians and more Africans than other groups.

Table 5. *Distribution of errors for mistaken one-race pairs*

Subject groups	Total errors	Percentage of inaccurate responses			
		European	Coloured	Indian	African
Afrikaners	51	5.9	23.5	39.2	31.4
English	46	4.3	37.0	56.5	2.2
Coloureds	70	8.6	51.4	40.0	0.0
Indians	44	9.1	29.5	61.4	0.0
Africans	163	6.7	35.0	54.0	4.3
Totals	374	7.0	36.1	50.5	6.4

Table 5 also shows interesting group differences in incorrectly judging non-European pictures as *Europeans*. The two white groups committed this error less than any of the three non-white groups. Also we note that the Coloureds and Indians tended to perceive their own race more than other subjects in their incorrect judgements.

(iv) *The perception of the two-race pairs*

In the previous section we have considered the judgements made of the one-race stimulus combinations. Let us now review the results of the six two-race stimulus combinations (5–10), the more ambiguous stimuli which do not represent any one race.

Table 6 summarizes the responses made to the ambiguous pairs. A 'suppression' is defined as the naming of *either* of the two races represented in the two-race pair. With the European-African stimulus combination, for instance, either *European* or *African* is scored as a 'suppression' response. A 'suppression' response means therefore that one of the two alternatives is selected for report. By contrast, a 'manifest fusion' response occurs when the observer reports a third (usually intermediate) race. For example, with the European-African combination, he reports either *Coloured* or *Indian*.

Table 6 reveals very little difference between subject groups in their overall responding to these two-race pairs of photographs. If, however, we examine within the various stimulus combinations we note several points of interest. The most striking of these occurs with the European-African combination; slightly over half of the responses made to this stimulus combination were manifest fusions—in other words, were judgements of *Coloured*

or *Indian*. This figure contrasts with the less than 20% manifest fusions for each of the other five two-race pairs. We note, however, that the European subjects—particularly the Afrikaners—gave more suppression judgements than did the non-European subjects in the case of the European-African mixture. Thus, as Table 6 shows, the Afrikaners gave 60% bifurcated (black or white) judgements as compared, say, with the Indian subjects who gave only 37.5% of such judgements.

The Afrikaners also vary from the other groups in their responses to the Coloured-Indian two-race pair. They tended to make fewer suppression judgements and consequently more *European* and *African* judgements to this stimulus combination than other subjects.

Table 6. *Percentage of 'suppression' identifications of ambiguous, two-race pairs**

Subject groups	N	Stimulus combinations						Total average
		E-C (5)	E-I (6)	E-A (7)	C-I (8)	C-A (9)	I-A (10)	
Afrikaners	20	77.5	90.0	60.0	88.8	91.3	95.0	83.8
English	22	87.5	88.6	56.8	95.5	88.6	94.3	85.2
Coloureds	20	83.8	86.4	45.0	98.8	92.5	93.8	83.4
Indians	20	91.3	98.8	37.5	97.5	97.5	88.8	85.2
Africans	40	71.3	93.1	45.6	91.9	95.6	83.1	80.1
Total average		80.5	93.0	49.2	94.1	93.4	89.8	83.3

* 'Suppression' is defined as a response of *either* of the two races represented in the mixed-race pair. For example, with the European-Coloured stimulus combination, either European or Coloured would be scored as a suppression response.

Table 7. *Distribution of manifest fusions of two-race pairs**

Subject group	Total manifest fusions	Percentage manifest fusions			
		European	Coloured	Indian	African
Afrikaners	78	5.1	37.2	41.0	16.7
English	78	0.0	44.9	42.3	12.8
Coloureds	73	1.4	39.7	56.2	2.7
Indians	68	2.9	55.9	38.2	2.9
Africans	191	6.3	39.8	47.6	6.3
Totals	488	3.9	42.4	45.7	8.0

* 'Manifest fusion' is defined as a response which names *neither* of the two races represented in a particular mixed-race pair of pictures. For example, with the European-Coloured stimulus combination, either Indian or African would be scored as a manifest fusion.

Additional evidence for this bifurcated tendency is seen in Table 7. One would normally expect manifest fusions (the naming of some race not represented in the stimulus pictures) to yield a judgement of *Coloured*—since this is the only legally designated mixed race in South Africa; or perhaps the judgement of *Indian* since in fact Indian photographs partake of dark complexions as well as European features of face and hair. All groups do in fact show this expected trend; 88% of the total manifest fusions are reported as either Coloured or Indian. At the same time, the Afrikaners, in whom we are especially interested, manifest this tendency less than other groups. Table 7 shows that they tend to report *Europeans* or *Africans* more often (22%) and *Coloureds* or *Indians* less often (78%) than other groups. We note also that the English-speaking subjects tend to resemble the Afrikaans-speaking subjects in their relatively large number of African manifest fusions (possibly evidence for perceptual vigilance).

(v) *Education and judgements of race*

We mentioned previously that the Coloured and African subjects were on the average somewhat less educated than the other three groups. When it was found that the Coloured and African subjects were also the least accurate in judging the one-race pairs (Table 4), the question was raised as to the relationship between education and accuracy. For the nineteen relatively well educated European subjects (eighth standard or higher) 86.2% of the one-race stimuli were judged correctly, while the six poorly educated European subjects (sixth standard or less) had 88.5% of their judgements correct. Though no educational effects were apparent in these European data, non-white subjects did evidence a very slight positive relationship. Seventy-nine per cent of the nineteen well educated non-European subjects' one-race judgements were accurate as compared to 77.3% of the judgements made by the poorly educated non-white subjects. Education seems only slightly if at all related to one-race accuracy in our stereoscopic situation.

There are still further indications in the data that the degree of education is not a crucial variable. Our special group of interest, the Afrikaners, revealed a number of interesting differences in their racial identifications from the English and Indian subjects, yet all three of these subject groups were fairly well educated. The unique results of the Afrikaans-speaking participants cannot be accounted for in terms of educational differences.

One difference did occur between the relatively well educated and the poorly educated subjects, both European and non-European. The better educated respondents tended to make more *Coloured* identifications than the relatively uneducated subjects. Twenty-six per cent and 29.4% of the higher educated European and non-European responses were *Coloured* as compared with the 21% and 25% figures of the poorly educated. The South African legal definition of a Coloured being any individual of mixed race means that those of our sample who were relatively well educated were in fact responding more accurately in a legal sense to the two-race stimulus combinations.

V. DISCUSSION

The use of meaningful stimuli in stereoscopic research presents many complexities. Photographs, particularly if not expertly done, are often slightly 'muddy' in appearance. We are inclined to believe that this source of error, for example, partly accounts for the unusually large number of *Indian* judgements made by our subjects. Moreover, limitations are introduced by tachistoscopic presentation. Two-second exposure such as we used makes it impossible to study the phenomena of rivalry and fluctuation as Engel (1956) succeeded in doing with longer intervals of presentation. An additional complexity is created by eye dominance. Unless adequate controls can be employed, the mere physical fact of eye dominance will seriously affect the results.

While our rapid exposure method limits the range of problems that can be studied, it does have certain advantages. We employed it in this study both to minimize fluctuation and to simplify communication with our subjects, with half of whom an interpreter was necessary.

Two methods controlled the eye dominance factor: (1) reversal of the order of

presentation of the stimuli to the right and left eyes, and (2) discarding the data of the 10% of our subjects who showed severe eye dominance.

In spite of the difficulties of stereoscopic research of this sort, we believe we have certain important findings. The present data clearly show that identification of race is a function of both the stimuli being perceived and the perceiver making the judgements. Let us consider each in order.

(i) *The stimulus as a factor in racial identification*

Recent studies in social perception have stressed perceiver variables almost to the exclusion of stimulus factors. Our data, however, indicate that the largest differences in racial identification are due to differences in the stimuli. First, it should be remembered that our entire subject population of 122 persons is correct in judging the one-race pairs on the average of four out of five times. For reasonably unambiguous stimuli, then, the racial identifications of our subjects are generally accurate (Table 4).

Another important point concerns the discriminability of the four races employed in this investigation. As represented by our photographic stimuli, the four groups are differentially difficult to judge correctly. Table 4 indicates that the one-race pairs of Africans are named nineteen out of every twenty times, and the one-race pairs of Europeans are correctly named eight out of every ten times. Indians and Coloureds, particularly the latter, are more difficult to judge accurately.

The two most accurately perceived groups—Africans and Europeans—are the most distinctly different in appearance of the races used. Table 6 shows that the stimulus combination of African and European is judged in terms of fusion far more than any other two-race combination. That is, over half of the judgements made of the African-European mixture are either *Coloured* or *Indian*; all other stimulus combinations involve naming at least four out of every five times, one of the two races actually represented. Such racial cues as skin colour, hair texture, and facial features are apparently in great conflict in the European-African combination and the conflict tends to be resolved with an intermediate judgement involving a 'mixed-cue' race. Europeans and Africans can be thought of as polar-opposite 'anchors' for the racial identifications of our subjects.

Indians and Coloureds are intermediate, mixed-cue races. They are the most difficult to judge, blending as they do features of both of the anchoring races. They are also the most frequently employed racial labels—particularly in response to ambiguous stimuli. Table 2 shows the disproportionately large use of the *Indian* and *Coloured* when judging two-race pairs; 64% of all judgements being either *Indian* or *Coloured*. Of the errors made in response to the one-race pairs, Table 5 shows that 87% are either *Indian* or *Coloured*. Moreover, Table 7 reveals that when fusion of the two images is manifest in the response to two-race stimuli, 88% of these fusions are either *Coloured* or *Indian*.

An additional point should be made concerning the greater frequency of *Indian* and *Coloured* judgements. By South African law, all racial mixtures are Coloured. The legally accurate response to all of the two-race stimuli should have been, therefore, *Coloured*. The fact that more *Indian* than *Coloured* judgements were made (Table 2) suggests that other cues—not legal definitions—are of importance in determining racial perception. Both the unusually frequent occurrence of *Indian* judgements (Table 2) and the more accurate judging of Indian as compared to Coloured one-race pairs (Table 4) may be due in part to the fact that Durban, the locale of this study, contains roughly eight Indians

to every one Coloured. Due to greater familiarity with Indians, our subjects might have tended more often to resolve a conflicting cue stimulus as an *Indian*, while persons in Cape Town—a city with many more Coloureds than Indians—might well have resolved the same conflicting cue situation by judging the stimulus mixture to be *Coloured*. Our present data do not allow us to estimate the importance of this factor of familiarity.

Summarizing, the racial identifications of our subjects varied considerably as a function of the stimuli. The European and African stimuli are more discriminable: these races are most dissimilar in appearance and social status; they seem to form polar-opposite anchor-age points for the identifications. The Indian and Coloured stimuli are more difficult to identify accurately. Moreover, *Indian* and *Coloured* judgements are more often employed in response to two-race stimuli and more often used when errors occur in judging one-race pairs. The Indian and Coloured stimuli, then, seem intermediate between the European and African anchors. Taken together, all of this evidence indicates that our subjects' racial identifications are in large part determined by the particular stimuli being judged.

(ii) *The perceiver as a factor in racial identifications*

Factors of directed state likewise play a part. Our guiding hypothesis predicted that members of different ethnic groups would vary in their identifications of each other. The possible operation of perceptual defence, perceptual vigilance, and bifurcation of judgements was expected.

Throughout the results, the Afrikaners deviate most consistently in their judgements from the other four groups. They tend to respond in an either-or bifurcated manner. Let us review the evidence.

Table 2 shows the tendency for our subjects to give more *Coloured* and *Indian* than *European* and *African* judgements—especially when responding to the ambiguous, two-race pairs. Afrikaners, however, differ from the other subjects in this respect. In Table 3 we note that they give the smallest percentages of *Coloured* and *Indian* and the largest percentages of *European* and *African* judgements of any group. (In giving their judgements Afrikaners refer to Africans as *Bantu*, *Native*, or *kaffir*. Part of the reason for these less courteous labels is that in the Afrikaans language the term *African* would be *Afrikaner*!)

This bifurcated (black-white) responding of Afrikaners is further illustrated in their perception of one-race pairs. While moderately accurate in recognizing Indian and African one-race pairs, Afrikaners are the most accurate in judging European one-race pairs and the least accurate in judging the Coloured one-race pairs (Table 4). Even more interesting is the Afrikaners' distribution of errors for mistaken one-race pairs (Table 5). When they incorrectly identify one-race pairs, they make less *Coloured* and *Indian* judgements than any other group, and twice as many *African* judgements as all other groups combined. In short, Afrikaners tend to be comparatively accurate in judging one extreme—the Europeans, while frequently employing the other extreme—the Africans—as the judgement for all other one-race pairs.

The responses made to the two-race stimuli by the Afrikaans-speaking subjects furnish additional evidence for the bifurcation tendency. When judging the European-African mixtures, the Afrikaners tend to suppress one of the images and report either *European* or *African* more than any other group (Table 6). Similarly, when viewing the two-race pairs involving the two 'intermediate' races, Afrikaners again give more European and African judgements than the other four subject groups.

Table 7 provides further analysis of the viewing of the two-race pairs. In their manifest fusions (responding with a race not represented in the mixed pair), Afrikaners have a larger percentage of *African* and a smaller percentage of *Coloured* judgements than any other group.

The black-white, bifurcating judgements of our Afrikaner subjects were evident, then, in response to all of our stimulus combinations, as a glance back to Table 1 will confirm. Previous work in this area suggests several possible reasons for our results.

Some investigators have noted a bifurcating tendency in the judgements of highly involved subjects. C. Hovland and M. Sherif (1952), working with the sorting of items for a Thurstone-type Negro scale, found that 'Negro subjects and militantly pro-Negro white subjects tended to place a disproportionate number of statements in the extreme categories, in line with the predictions from judgment theory that individuals with strong attitudes tend to see issues in "black and white" and displace neutral statements to the extremes'. Later, Sherif & Hovland (1953) noted that subjects with instructions to use any number of categories they wanted tended to employ fewer categories in direct proportion to increased involvement in the statements being rated. In line with the bifurcation hypothesis, highly involved subjects generally require a minimum number of categories and ignore small differences between stimuli.

Additional interest has been generated in bifurcated judgements by the work of Adorno, Frenkel-Brunswik, Levinson & Sanford (1950) on the authoritarian personality. E. Frenkel-Brunswik (1949) first reported that authoritarians typically were 'intolerant of ambiguity' and responded in an either-or fashion. For highly authoritarian people, she writes, 'a simple, firm, often stereotypical, cognitive structure is required. There is no place for ambivalence or ambiguities. Every attempt is made to eliminate them, but they remain as potentials which might interfere at any time. In the course of these attempts a subtle but profound distortion of reality has to take place, precipitated by the fact that stereotypical categorizations can never do justice to all aspects of reality' (Adorno *et al.* 1950, p. 480).

Considerable evidence supports these views. Authoritarians disproportionately often agree with such categorical items as 'there is only one right way to do anything' and 'there are only two kinds of people: the weak and the strong' (Adorno *et al.* 1950). E. Roper (1946) found in analysing public opinion poll results that respondents relatively high in anti-Semitism typically gave either a 'yes' or a 'no' but rarely a 'don't know' answer when asked their opinion on current events. The racially apprehensive person apparently needs to give a definite response, not an ambiguous 'don't know'. Further support for this tendency has been summarized by G. W. Allport (1954, pp. 400-3).

It is difficult to ascertain from our data the predisposing factor underlying the bifurcating tendencies of our Afrikaner subjects. It seems reasonable, however, to assume that these results reflect the Afrikaners' heightened concern for, and deep involvement in, the racial issues of the Union of South Africa. Like Hovland and Sherif's subjects, they may be yielding black-white, either-or judgements more than other groups because of their greater involvement. The authoritarian personality research suggests the possibility that special personality factors may also have been operating to cause the bifurcating results noted in our data. More detailed study is required to assess these various possibilities.

Our data offer further indications that racial identifications are affected by the perceiver's own race. In Table 4, for instance, we note that for three out of the four races

one-race pairs are most accurately judged by the racial group represented. Afrikaans-speaking and English-speaking Europeans are the most correct in judging European pictures, Coloureds in judging Coloured pictures, and Indians in judging Indian pictures. Even the one exception, Africans, are more accurate in recognizing African pairs than they are in recognizing any other one-race pairs. Such a finding seems plausibly explained in terms of greater familiarity with one's own ethnic group.

Table 4 also shows that Coloureds and Africans are considerably less accurate in judging one-race pairs generally than are the other groups. Since education is not strongly related to accuracy in our data, exceptionally restricted contact with other races by our Coloured and African samples may be one possible reason for this finding.

Another group-related difference is seen in Table 5. When errors occur in judging one-race pairs, Coloured and Indian subjects tend to report their own race too often. By way of contrast with this hospitable orientation, the two European groups are most careful *not* to judge non-European pairs as European. This result must be partly a function of the difference in discriminability of the races noted in the previous section; Europeans being easier as a race to identify correctly than either Coloureds or Indians. But in addition, differential status between the races may also be a factor. As the high status groups, the Afrikaners and English have the most to lose by including others in their group and hence are the most discriminating concerning the use of the European label. Coloureds and Indians, on the other hand, being of lower status, are more willing to judge each other's pictures or European pictures as belonging to their own group. A glance back to Table 1 shows us that our Coloured and Indian subjects sometimes judge the European one-race pairs as their own race, but never judge their own one-race pairs as African—the lowest status group of all.

A final relationship concerns education and the number of *Coloured* responses. As has been mentioned, all of the two-race mixtures should legally be designated as Coloured. Thus, the tendency for the relatively well educated subjects—both European and non-European—to give more *Coloured* responses is in the direction of technical accuracy. The difference, therefore, may reflect the wider knowledge better educated people have concerning the legal situation.

It seems clear from a review of these findings that our general hypothesis is supported: racial identifications are determined in part by subjective variables and especially by the subject's own racial membership.

VI. CONCLUSIONS

The use of meaningful pictorial stimuli in a setting of binocular rivalry proves to be a promising technique for exploring the determinants of 'social perception', especially those sets often called *defence* and *vigilance*. In this paper we offer no particular theory of cognition to account for the results, but are content to establish the fact that cultural memberships, expectancies, and subjective attitudes (directed states) play some part in the resolution of binocular conflict. Important for any theory is the rapidity with which the resolution occurs employing both objective and subjective cues. In less than 2 sec. our subjects reach a decision with high subjective certainty concerning which one of four races is represented by the binocular stimuli, even in those cases where two *different* races appear before the two eyes. There is no awareness of binocular conflict; a single judgement is promptly given. While we cannot know the precise course of the complex

weighting of objective and subjective cues, nor the exact process by which fusion (or suppression) takes place, we nevertheless clearly demonstrate the presence of what M. D. Vernon (1957) calls 'cognitive inference in perceptual activity'.

When both eyes receive the image of two different members of the same race (and sex) the great majority of subjects give correct identifications. The accuracy is greatest for the two most discriminable groups—the Europeans and the full-blooded Africans. When different races are presented to the two eyes they are seen most often as 'intermediate', Coloureds or Indians. But it is in the departure from this modal tendency (veridicality) that the subjective determinants are evident. In general, observers are most accurate in judging members of their own group; and we show that familiarity with any group has favourable effects on identification. When errors are made, they are often of such an order as to preserve the status of one's own group.

Our most striking evidence for the role of directed states in binocular resolution comes from the tendency of Afrikaans-speaking whites to sort their judgements into two categories—white and black. While the number of our Afrikaner subjects is small, the trend finds some support from the similar but less pronounced results among English-speaking South African whites. Broadly speaking both groups feel threatened by the non-European majority, the largest segment of which is the African Bantu. To maintain white supremacy it seems functionally useful for Europeans to regard all non-Europeans as 'blacks', since in this direction the 'menace' lies. Intermediate perceptions would, so to speak, simply distract one from the main racial threat. In view of the more intense convictions of Afrikaners (on the average) in favour of white supremacy it comes as no surprise that their tendency toward perceptual bifurcation is more marked than among the English-speaking South Africans who, on the whole, are ideologically less tense.

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ATTENTION TO VISUAL DISPLAYS DURING A VIGILANCE TASK

I. BIASING ATTENTION

By C. H. BAKER

Applied Psychology Research Unit, Medical Research Council, Cambridge

Five experiments are described in which subjects were asked to search visual displays and report the appearance, at any location on the display, of a spot of light which persisted for one second. On displays with and without a rotating radial line it was found that subjects detect fewer such spots of light near the periphery of the display. By modifying a radar-like display in order to facilitate a changed pattern of visual search, it was demonstrated that more peripheral spots could be detected.

I. INTRODUCTION

Remarkably little is known concerning techniques for scanning visual displays on which a signal may appear at any point with equal probability. Visual search is of extreme importance in the case of radar displays when employed to give warning of approaching ships or aircraft. The type of display chosen for study here was a P.P.I. (Plan Position Indicator) which is not unlike a circular TV screen, though the 'picture' of the area covered is 'painted on' by a revolving radial line of light. The 'picture' fades in a few seconds, only to be 'repainted' on successive revolutions of the radial line. A target is represented by a small spot of light (a pip) brighter than the background 'picture'. Range of a target so displayed is represented by the distance of the pip from the centre of the P.P.I., a target at maximum radar range being represented by a pip at display periphery. Bearing to the target is represented by compass marks around the periphery. Radar operators must continually search for pips. That search behaviour of operators is far from optimum is indicated by the fact that approaching targets are often not detected until their associated pips are well in towards mid-radius of the display. Such 'peripheral blindness' results in decreased warning when targets are approaching. The present studies were undertaken to determine (1) the nature of visual search behaviour in a radar-like situation, and (2) ways of biasing search behaviour in an attempt to overcome 'peripheral blindness'.

II. METHOD

There is, to date, no reliable way of recording eye movements over prolonged periods while subjects remain physically free, i.e. physically independent of apparatus. However, pertinent data can be obtained in a free setting by injecting signals onto a display at desired locations and having subjects respond whenever a signal is seen. This was the method employed. Subjects sat in a darkened cubicle, 34 by 40 in., facing an opal glass screen set into the narrow side of the cubicle. A steady masking noise, (58 db.), and air circulation were provided by an exhaust fan. From outside the room a circle of light 12 in. in diameter was projected onto the screen. In all experiments a pip (4 mm. diameter) appeared at predetermined intervals for 1 sec. at any one of the 16 locations shown in Fig. 1. Luminance of the projected circle was 12 foot-lamberts, while that of the pips was 14.5 foot-lamberts. When seated upright in their fixed chair, subjects' viewing distance was 18 in. Display conditions were changed slightly from one experiment to the other, though these were the ones employed in Exp. 1.

In each experiment sixteen subjects were employed, each for 64 min. A latin-square design permitted each subject to be presented with a pip at each of the sixteen locations in random order of succession

during the first 32 min. The second 32 min. period was a replication of the first. (In all, eighty R.N. ratings, sixteen in each experiment, were employed as subjects. None of them had radar experience with a conventional P.P.I. All were given sufficient practice to ensure that they thoroughly understood the nature of their task.)

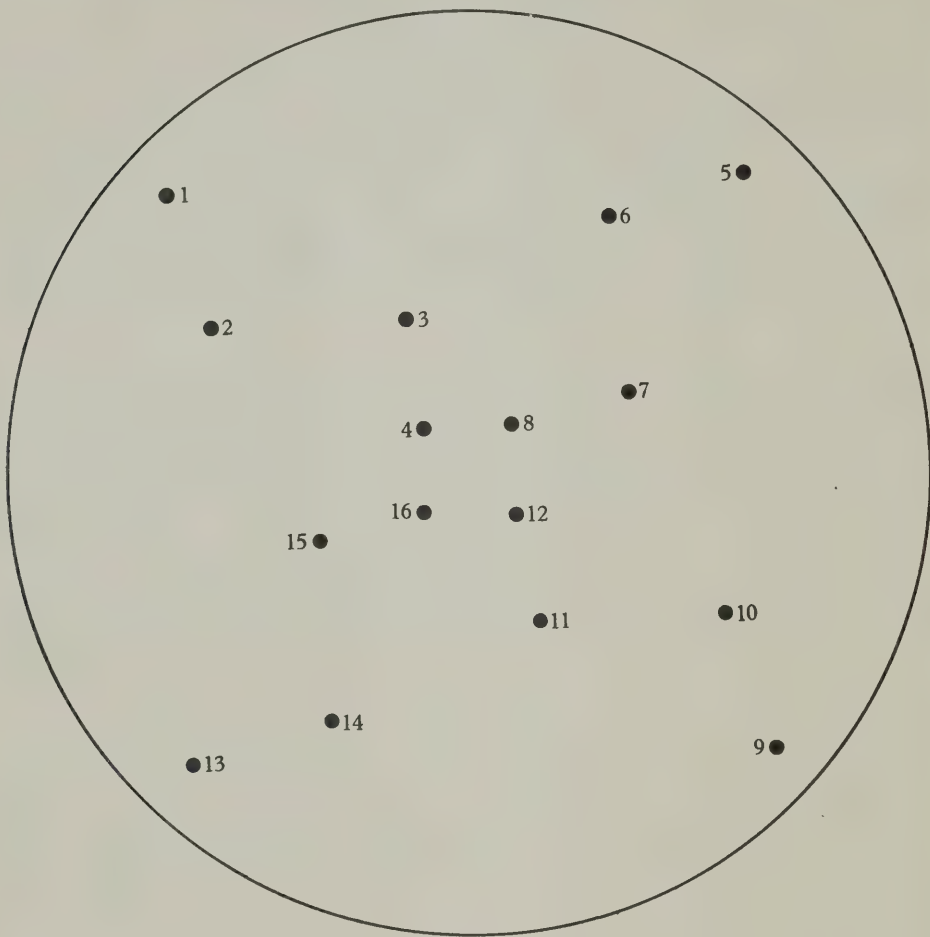


Fig. 1. Showing the sixteen locations at which the pip appeared.

Though pips appeared on an average of once every 2 min., the actual intervals between appearances ranged randomly between 36 and 196 sec., as follows: 1:58 (1 min. 58 sec. from the beginning), 3:34, 5:42, 7:04, 9:50, 11:38, 12:40, 15:36, 17:00, 18:38, 21:28, 22:44, 26:00, 26:36, 29:52, 31:30, and repeat, i.e. 33:58, etc. The subject's task was simply to search the display for the appearance of a pip and to press a switch (giving response time on a clock) whenever one was seen. They were not informed of how many pips would be presented, or where they would appear, though they were informed that the task lasted 64 min. Watches were removed.

Because the data from the five experiments reported here were later pooled for analysis purposes, the experiments are individually described below with a brief summary of results, followed by a section giving the pooled results.

III. EXPERIMENTAL RESULTS

Experiment 1. Blank display (no sweep-line)

Exp. 1 was designed to determine the distribution of visual attention to the display in the absence of any form of rotating radial sweep-line—see inserted figure for Exp. 1 in Fig. 2. Sixteen naval ratings with no experience in radar operation served as subjects. The data from Exp. 1 (see Fig. 2) indicate that

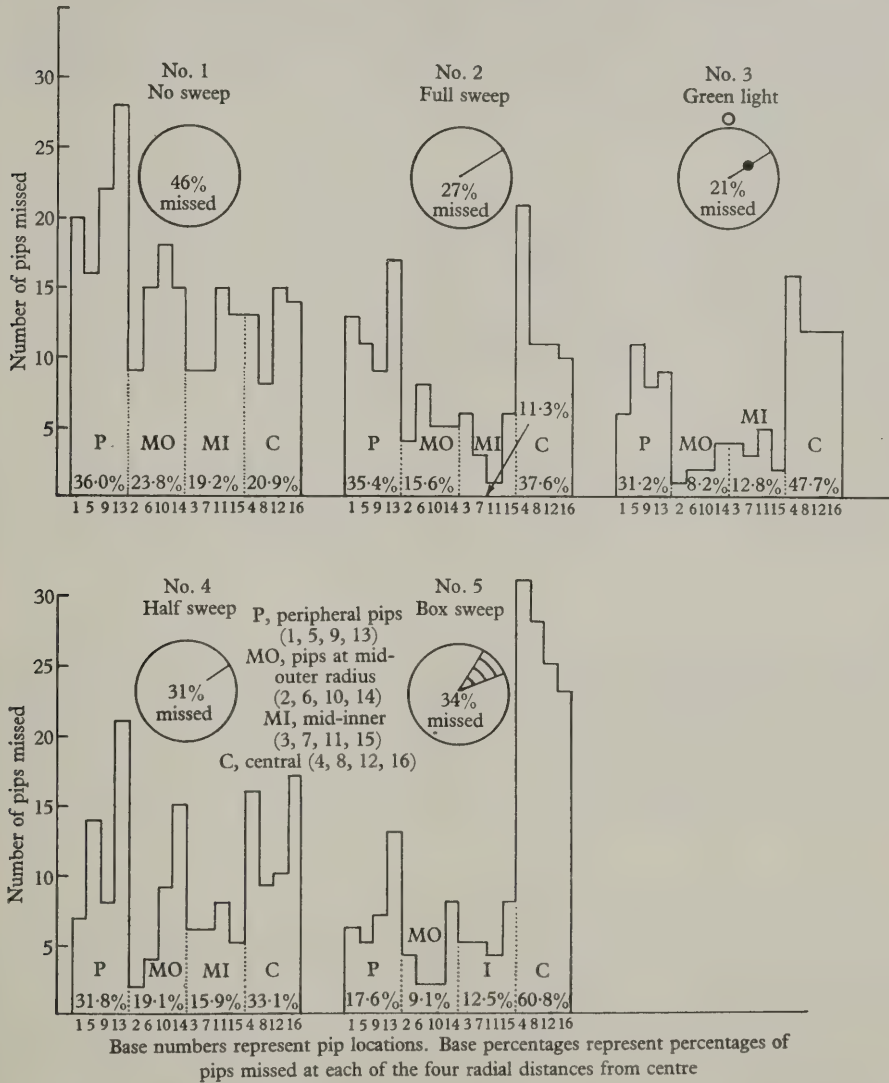


Fig. 2. Showing, for each of five experiments, the number of pips missed at each of sixteen locations. The base percentages refer to the percentage of pips missed at each of four radial distances from the centre of the display.

46% of the pips were missed, and the histogram shows the manner in which these misses were distributed for each of the sixteen locations. The greater proportion of failures to detect pips, 36%, were those located near the periphery (P). The remainder of those not detected were quite evenly distributed over the middle-outer (MO), middle-inner (MI), and central (C) regions. (Unfortunately, we know of no statistical

test which can be used to determine whether or not there are significant differences between the number of failures in detection in the different regions. However, an analysis was performed on response times, as will be shown.) Apparently there is some 'peripheral blindness' when viewing even a blank display.

Experiment 2. Conventional P.P.I. display (full sweep-line)

In Exp. 2 a radial line (sweep-line) was introduced which revolved at 6 r.p.m. Pips were 'painted on' now by the sweep-line—see inserted figure in Fig. 2. Subjects therefore had a much smaller area to search i.e. immediately behind the line, and so it was anticipated that fewer pips would remain undetected. The data are shown in Fig. 2; 27% of the pips were not detected, but these were distributed differently from those in Exp. 1. As shown, there was a marked 'peripheral blindness' (35.4% of those undetected), but also a marked 'central blindness'—37.6%. Pips at approximately half radius were detected about twice as often as peripheral or central pips.

Experiment 3. Green light

From the data in Exp. 2 it would appear that subjects tend to search the area behind the mid-radius of the sweep-line with relatively occasional deviations to the extremes of the line from the preferred mid-areas. Exps. 3–5 introduced modifications to the display which were intended to bias subjects' search technique in favour of the peripheral regions of the display. A small dot was added to the sweep-line at exactly half radius. In addition, a small green light was positioned an inch above the display; see inserted figure in Fig. 2. The green light came on for randomly determined intervals of 2, 3, or 4 min., and was then off for any one of these periods. During the 64 min. it was on for a total of 32 min.

Subjects were instructed that when the light was off they were to search behind all portions of the sweep-line for pips (as in Exp. 2), but when it came on they were to search only from the spot to the periphery. Thus about three-quarters of the 64 min. period would be spent in searching the outer half of the display. The data (Fig. 2) show that only 21% of the pips were not detected and that visual attention was indeed biased in the required direction, i.e. 31.2% of those not detected were peripherally situated versus 47.7% centrally located.

Experiment 4. Half-sweep

Could greater bias be obtained than that achieved in Exp. 3? Since subjects display a preference for the region behind the mid-radius, then if only the outer half of the sweep-line was displayed subjects should continue to detect twice as many pips behind the mid-region of the half sweep-line—in this case peripheral pips. The data (Fig. 2) reveal that 31% of all pips escaped detection, 31.8% of these being peripheral compared with 33.1% in the central regions. And the misses near half radius were more numerous than those obtained in Exps. 2 and 3. These data were interpreted to mean that the conclusion from Exp. 2 was in error; subjects do not tend to search the area behind the mid-radius of the sweep-line with occasional deviations to the extremes.

Experiment 5. Box-sweep

The results of Exp. 4 led to the formulation of the following hypothesis. Subjects continuously search back and forth behind the line, having no particular preference for any area. With reference to Fig. 3A, let it be assumed that subjects employ four visual fixations (1, 2, 3 and 4) to 'cover' the area immediately behind the sweep-line once. Then, on returning, they use their 5th, 6th, and 7th fixations, at which point they reverse, using their 8th, 9th, and 10th fixations to reach the periphery again. It is readily apparent that over any prolonged period of time almost twice as many 'looks' are given to the mid-regions as to the extremes. The expected result would be half as many pips missed in these mid-regions, as found in Exps. 2–4. In order to distribute the 'looks' evenly it would be necessary to start the second and subsequent series of four 'looks' from the *same end* of the line each time—see Fig. 3B.

A box-sweep formed of two sweep-lines and three arc lines was employed as shown in Fig. 2. The second ('ghost') sweep-line was 40° behind the leading line. Subjects were instructed as follows: 'Search in the big box, the next, the next, and then the smallest. Then start again from the big box, and so on.' The data are shown in Fig. 2. Although a total of 34% of the pips were not detected, only 17.6% of them were peripheral compared with 60.8% in the central region. The remarkably large percentage of missed detections near centre remains unexplained.

In summary we can say that the green light technique (Exp. 3) resulted in fewest missed detections while the box-sweep resulted in greatest peripheral bias. Considering the *number* of peripheral misses only, there were thirty-four in Exp. 3 and thirty-one in Exp. 5, a non-significant difference.

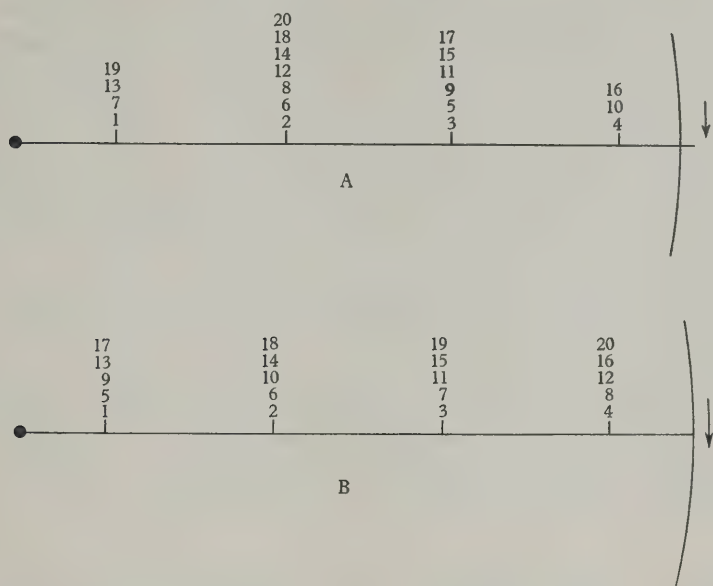


Fig. 3. (A) shows the hypothetical manner in which successive visual fixations are distributed in visually searching back and forth behind the sweep-line. (B) shows the distribution of fixations when each search is started from the same end of the sweep-line.

IV. POOLED RESULTS

The brief results given above are in terms of percentages of pips not detected. The major analysis was performed on response times. Response times for the five experiments were first transformed to log values. The greatest response time was 2.95 sec. and so the 'no-response' scores (pips not detected) were credited with a response time of 3.00 sec. Variances of each replication of each experiment were then analysed. The levels of significance obtained are summarized in Table 1.

Table 1. *Showing the levels of significance obtained from the ten variance analyses of the first and second replications (R1 and R2) for each of the five experiments*

Source of variation	No sweep		Full-sweep		Green light		Half-sweep		Box-sweep	
	R1	R2	R1	R2	R1	R2	R1	R2	R1	R2
er	0.05	N.S.	N.S.	N.S.	N.S.	N.S.	0.05	N.S.	N.S.	N.S.
ects	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
radii	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
azimuth	0.01	0.05	N.S.	0.05	N.S.	N.S.	N.S.	0.05	0.001	N.S.
ii/azimuth	N.S.	N.S.	N.S.	N.S.	N.S.	N.S.	0.05	N.S.	0.01	N.S.
eraction										

It is apparent from Table 1 that range/azimuth interactions occur only twice, each during a first replication. A separate analysis revealed that for the half-sweep the interaction was due to a disproportionate number of missed detections near centre in the

bottom quadrant, while in the box-sweep there were disproportionately more misses near centre in both the bottom quadrants.

The major sources of variation are subjects and pip radii. Azimuth position of pips is significant in only half the replications: in the two cases where azimuth is highly significant (no-sweep, R1, and box-sweep, R1) the level of significance is less in the second replications (0.05 for no-sweep), or nil in the case of the box-sweep. A separate analysis, not shown here, was undertaken to determine the nature of these azimuth differences. It was found that in the case of no-sweep the significantly longer response times were for the bottom left and bottom right quadrants in both replications. For the full-sweep the significantly longer times were in the top-right quadrant, while for the half-sweep and box-sweep the significantly longer times were for the bottom-right, and for the bottom-right and bottom-left quadrants, respectively.

In only two of the replications is order significant, both at the 0.05 level. In both cases this level of significance was obtained in the first half of the task, becoming nil in the second.

Before pooling the data from the five experiments Bartlett's test of homogeneity of residual variance (Edwards, 1950) was applied to the residual variances obtained by the analyses summarized in Table 1. The differences were found to be not significant, i.e. gave no evidence against the hypothesis of random sampling from a population having a common variance. Accordingly, the data were pooled, resulting in the analysis given in Table 2.

Table 2. *Variance analysis of the data from the five experiments combined*

Source	D.F.	S.S.	M.S.	F	P
Experiments (E)	4	9.773	2.4433	8.339	0.001
T $\times$ L (between)	15	6.196	0.4131	1.409	N.S.
Error (between)	60	17.577	0.2930	—	—
Subjects (S)	79	33.546	0.4246	17.052	0.001
Replications (R)	1	0.396	0.3960	3.535	N.S.
Trials (T)	15	0.675	0.0450	1.034	N.S.
Locations (L)	15	12.169	0.8113	5.879	0.001
T $\times$ L (within)	210	6.621	0.0315	1.265	0.05
T $\times$ E	60	2.609	0.0435	1.747	0.01
L $\times$ E	60	8.278	0.1380	5.542	0.001
R $\times$ E	4	0.448	0.1120	4.498	0.01
R $\times$ L	15	0.434	0.0289	1.161	N.S.
R $\times$ T	15	0.442	0.0295	1.185	N.S.
R $\times$ L $\times$ T	225	5.813	0.0258	1.036	N.S.
R $\times$ E $\times$ T	60	1.295	0.0216	0.867	N.S.
R $\times$ L $\times$ E	60	1.429	0.0238	0.955	N.S.
E $\times$ L $\times$ T (within)	840	26.613	0.0317	1.273	0.01
Error (within)	900	22.452	0.0249	—	—
Total	2559	123.220	—	—	—
Azimuth (A)	3	0.638	0.2127	1.541	N.S.
Radius (Rad)	3	10.015	3.3383	24.190	0.001
A $\times$ Rad	9	1.516	0.1684	1.220	N.S.

Starting with the larger interaction terms, it is apparent from Table 2 that $E \times L \times T$ is highly significant. We interpret this to mean that there is indeed a systematic $E \times L \times T$ interaction, or an $S \times L \times T$ interaction, or both. Despite this significance it is apparent from the F -ratio that neither effect is large in comparison to the experimental error. The three remaining second-order interactions are not significant, showing that $R \times L \times E$, $R \times E \times T$, $R \times E \times L$, and the subject interactions $S \times R \times L$ and $S \times R \times T$ are not significant. The non-significance of the $R \times T$ interaction supports the view that the $S \times R \times T$

interaction is not significant, while that of the $R \times L$ supports the view that $S \times R \times L$ is not significant.

The $R \times E$ interaction, being significant, suggests that either there is a systematic $R \times E$ interaction, or an $S \times R$ interaction, or both. Unfortunately, we cannot distinguish between these possibilities. Again, the $E \times L$ interaction being significant, could also imply an $S \times L$ interaction.

The significance of the $T \times E$ interaction could be due, too, to an $S \times T$ interaction, while that of the $T \times L$ (0.05 level) could be due to an $S \times T \times L$ interaction.

It was noted above ($L \times E$ interaction) that an $S \times L$ interaction may exist. Consequently we have tested locations (L) against $L \times E$, yielding an F -ratio of 5.879. Evidently the effect of pip location is very marked. This location effect has been further analysed from azimuth, radius, and $A \times \text{Rad}$ interaction, as shown in the lower portion of Table 2. From this analysis it is readily apparent that the significant effect of location is almost totally due to radius.

With reference to the significance of T (Trials), it was noted that an $S \times T$ interaction might exist. Hence, we have tested T against $T \times E$. The resulting F -ratio of 1.034 is not significant. And for the reason that an $S \times R$ interaction might exist we have tested R against $R \times E$. The obtained ratio of 3.535, being non significant, indicates no effect of replications. The effects of subjects and of experiments are both highly significant.

In summary, then, it can be said that when response times for the five experiments are analysed together, experiments, subjects, and locations are highly significant factors, with one aspect of location, pip radius, contributing more to the variance than any other factor.

V. DISCUSSION

Distribution of attention

While a few studies have been made of visual scan techniques when regarding displays having certain static attention-getting features, e.g. Brandt's (1945) data using pictures, and the study by Fitts, Jones & Milton (1950) using aircraft instrument displays, the present studies are the only ones known to the writer in which the appearance of a signal at any point on the display is equally probable. Here we have subjects in a free setting, searching a display on which a brief signal *might* occur at any location.

The evidence from Exp. 1 is that when viewing a blank display subjects tend to disregard the periphery, while distributing their attention fairly evenly over the remainder. When 1 or 2 min. have elapsed since the preceding signal, Broadbent (1950) found that when watching a horizontal line of twenty dials, 'the centre of the display is more closely observed than the ends'. Broadbent's subjects reversed this tendency when signals were more than 5 min. apart. Broadbent's signals, however, remained on until detected. In another dial-watching experiment Lincoln & Averbach (1956), too, found that signals on centrally located dials were detected more often than those peripherally located, when the dials were tachistoscopically exposed.

These two experiments offer confirmatory evidence of 'peripheral blindness' when searching for a possible event. Does the same phenomenon obtain when searching for one among a number of events simultaneously displayed; for instance, when searching for one of a large number of clearly legible digits? This problem was investigated by Green, McGill & Jenkins (1953), who concluded that, 'certain numbers in a configuration are

easier to find than others', but they were unable to analyse for location. Accordingly, we undertook a sixth experiment, as follows.

A black circle having an area of 100 sq. in. was drawn on white cardboard. In the circle were printed 100 two-figure digits, 00-99, one pair of digits being randomly placed within each square inch of the display. All digits were oriented in the same direction. Sixteen pairs of digits were selected having the same relative locations as the pips in Exps. 1-5 (Fig. 1). Thirty subjects were individually tested to determine the time taken to locate these numbers, which were given one at a time. A variance analysis of the log values of the obtained times showed that subjects took significantly longer (0.05 level) to find the four peripherally located numbers than the remaining twelve.

Hence, it is argued that the phenomenon of 'peripheral blindness' characterizes visual search techniques when searching for an event or when decision taking, i.e. selecting one from a number of events according to some given rule.

It was shown in Exp. 2 that when a rotating sweep-line was introduced attention was distributed along the area behind the line in a manner reminiscent of a normal distribution. (It must be noted that the speed of rotation of the line is an important factor: at speeds too great for the eye to 'follow' the distribution would doubtless have been different.) Here, as indicated, the search area was much smaller than in Exp. 1. Buckley, Hanes & Deese (1953) have shown that probability of pip detection increases with decrease in area of search, hence the greater number of detections in Exp. 2. Because the area of pip appearance (and of visual search) was immediately behind the line it is reasonable to conclude that 'peripheral blindness' characterizes this smaller area too, i.e. 'blindness' at the extremes of the line rather than of the whole display. It appears that the reason radar operators often fail to detect targets at extreme radar range is simply that they do not distribute sufficient visual attention in peripheral display regions.

Smith & Boyes (1956) have since confirmed the phenomenon of 'peripheral blindness' on an actual radar display. They reproduced the display conditions of Exp. 2, using a 12 in. cathode-ray tube and a sweep-line revolving at 6 r.p.m. Pip thresholds (*detectability* thresholds) were determined by increasing the signal voltage 1 db. on each rotation of the sweep-line until the pip was detected. In addition they investigated three search areas—full display, half display, and quarter display. That is, subjects were informed that the pip would appear anywhere on the scope, in some particular half, or in some particular quarter.

The results are shown in Fig. 4. It is quite apparent that pip intensity must be markedly greater if it is to be detected near centre or periphery than if near half-radius. (In addition it should be noted that detection was better—threshold signal intensity lower—when only half or quarter of the display was being searched.)

The problem remained of determining whether this distribution of attention was due to subjective behaviour or to some characteristic of the cathode-ray tube. Accordingly *visibility* thresholds were obtained using the same cathode-ray tube, i.e. subjects were informed as to precisely where the pips would appear and threshold voltages were again determined. The result is shown in the top curve of Fig. 4. The slight drop in threshold voltage near centre remains unexplained. Of greatest importance, however, is the fact that *visibility* thresholds near the periphery were just as low as those near mid-radius. We are therefore led to conclude that the results of Exp. 2 can indeed be extrapolated to the real radar setting.

Exps. 3 and 5 demonstrated that the distribution of attention to such displays can indeed be biased in a desired direction—that ‘peripheral blindness’ can be significantly decreased by relatively slight modifications of the display. It should be noted here that in instructing subjects how to search in the green-light or box-sweep experiments we have no objective evidence that they did in fact do as instructed, though all subjects, when later questioned, maintained that they did. Furthermore, it is evident from an analysis

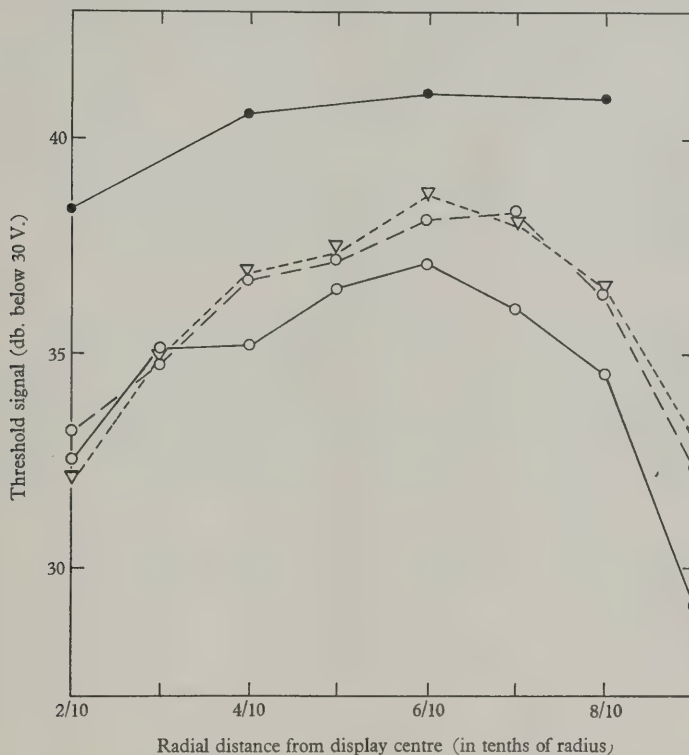


Fig. 4. Showing detectability thresholds for three areas of search, and visibility thresholds (top curve), as a function of radial distance of a pip from the centre of a radar display. The ordinate values are in decibels attenuation of a reference voltage: the greater the value the lower the threshold. Visibility, ●—●. Detectability: ○—○, full scope; ○—○, half scope; ▽---▽, quarter scope.

not given here that they maintained their search techniques, as instructed, throughout the hour—the histograms in Fig. 2 are representative of both replications in each of these two experiments. An unexplained feature of Exp. 5 (box-sweep) is the large number of misses in the central region. One possible reason is that because this inner box is the smallest it was ‘searched’ by only a single visual fixation. Another suggestion is that triangular displays are given less visual coverage than are those with four or more sides.

The data from Exp. 5 support our hypothesis. During a radar-like task subjects search back and forth in the area where pips are likely to appear. This method of search has been termed ‘search by parallel sweeps’ by Koopman (1956).

VI. CONCLUSION

When viewing a blank display on which a signal might occur at any location, subjects detect fewer signals occurring near the periphery of the display. Such 'peripheral blindness' obtains, too, when searching for one of a display of digits. In a radar-like task where signals might occur behind a rotating radial line, 'peripheral blindness' occurs near the two extremes of the line: signals near mid-radius are detected about twice as often. Visual attention can be biased toward the display periphery by changing the pattern of visual search.

This study was undertaken, while the author was on a tour of duty with the Medical Research Council, from the Defence Research Medical Laboratories, Defence Research Board of Canada, in Toronto. The work was done under the general direction of Dr N. H. Mackworth, and generous statistical advice was given by Mr M. Stone.

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EXPLORATORY BEHAVIOUR

By S. A. BARNETT

Department of Zoology, Glasgow University

A critical analysis is presented of some features of exploratory behaviour, especially in wild and tame rats but also in other species. Such behaviour is considered in the light of certain general concepts, especially 'appetitive behaviour', 'reactive inhibition', latent or exploratory learning, and motivation. The avoidance of new things, displayed by wild rats, is also discussed.

I. INTRODUCTION

That laboratory rats are highly exploratory and 'inquisitive' has long been known, though the importance of this behaviour has only gradually been realized. Small (1899) described the restlessness of infant rats as 'premonitions of curiosity', and remarked that fully developed exploratory behaviour, evident from the age of 3 weeks, is not merely a reflexion of hunger. In the modern period of behaviour study, among the first important observations were those of Blodgett (1929): this work was the first of a series of studies, by various authors, on 'latent learning', demonstrating that the exploratory movements of rats are often accompanied by topographical learning which is independent of evident reward. Reviews of this subject have been published by Majorana (1950), Munn (1950, pp. 315 *et seq.*), Thistlethwaite (1951) and Thorpe (1956, pp. 90 *et seq.*). Apart from workers concerned with latent learning, many psychologists have remarked in passing on the exploratory propensities of white rats (e.g. Buytendijk, 1931). Occasionally, this type of behaviour has obtruded itself where something else was sought: Locke (1936), for instance, looking for evidence of a 'social drive', found instead marked indications of an 'exploratory drive'. Since 1945 the study of exploration has been given more attention, and this recent work provides much of the substance of the discussion below. Independently of psychologists working with laboratory rats, zoologists have begun to study wild rats, both in the field and in cages or enclosures. While psychologists have, on the whole, used rats as 'preparations' showing special phenomena such as latent learning or spontaneous alternation, zoologists have been concerned with *Rattus norvegicus* Berkenhout (the species to which tame rats also belong) as an animal which maintains itself in certain environments (e.g. Chitty & Shorten, 1946; Calhoun, 1949; Barnett & Spencer, 1951; Chitty, 1954). Further, by using wild rats, they have been able to study a greater range of behaviour than is shown by tame, laboratory-bred animals: in particular, wild rats display a type of avoidance behaviour not observed in tame ones (Chitty & Shorten, 1946; Shorten, 1954; Barnett, 1956, 1958*b*). The object of this paper is to present a critical analysis of some features of exploratory behaviour, especially in wild and tame rats but also in other species; and to relate this to certain general concepts, especially 'appetitive behaviour', 'reactive inhibition', latent or exploratory learning and motivation; the avoidance of new things, displayed by wild rats, is also discussed. Exploratory behaviour is of great importance in many, perhaps most, animal species. It has been studied in a cockroach, *Blattella germanica* (Darchen, 1952, 1955), the hive bee, *Apis mellifera*, (Lindauer, 1953), the rhesus monkey, *Macacca mulatta* (Butler, 1953, 1954; Butler &

Alexander, 1955; Harlow, Blazek & McClearn, 1956), and the chimpanzee, *Pan troglodytes* (Welker, 1956*a, b*). But the most detailed and extensive studies have been those on rats, and it is these that will be principally drawn upon in this review.

II. 'APPETITIVE' AND EXPLORATORY BEHAVIOUR

(1) Definition of 'appetitive'

Many animals perform variable (unpredictable), sometimes apparently random, movements before they reach a situation in which they can carry out a stereotyped act such as eating. The name given to such movements is *appetitive behaviour*; a formal definition of this term in common use is: 'the variable introductory phase of an instinctive behaviour pattern or sequence' (Thorpe, 1951). This type of behaviour has been very fully discussed by Thorpe (1956, pp. 36 *et seq.*); as he points out, the activities so defined are exceedingly diverse and, in their relations to the various stereotyped acts, very complex. A simple example is the quartering of its territory by a predatory bird, before a victim is seen, pursued and captured; but not all appetitive behaviour is on this scale, since the movements, within a small compass, of, say, a bird shifting about before settling in its nest, also come within this class of actions. In the discussion below only the larger-scale movements about the animal's living space are considered, but this is for convenience only.

The definition given above reserves the term 'appetitive behaviour' for movements in an *instinctive* sequence. This would presumably exclude random movements preceding any activity in the development of which learning had taken a substantial part. For instance, Seitz (1940) brought evidence that in Cichlid fish of the genus *Tilapia* the males do not have an inborn ability to recognize conspecific females, but that they learn to do so during early life. If this is so, then courtship by the males of this genus depends on learning, and it might be questioned whether this particular behaviour should be called 'instinctive'. Admittedly, in this example the motor performance of the male is stereotyped for a given species; but in other instances it may be that even stereotyped action patterns, such as bird song (Thorpe, 1954*b*), or reproductive behaviour in rats (Beach, 1951), can be performed only if the animal has had the opportunity to learn them. Another way of putting this point is to say that definitions of the term 'instinctive behaviour' beg the question of just how the behaviour referred to develops in the ontogeny of the individual (cf. Lehrman, 1953): consequently, it is sometimes difficult to decide whether a specific behaviour pattern comes within the scope of the definition. As far as defining 'appetitive behaviour' is concerned, the obvious way out of this difficulty is to omit the word 'instinctive', and so to use 'appetitive behaviour' to refer to variable movements preliminary to any stereotyped activity, regardless of the extent to which 'learning' has played a part in its development.

However, even when the definition is shorn of this particular ambiguity, a serious difficulty remains in describing variable behaviour as it is actually observed. This is because behaviour which, while it is going on, is indistinguishable from appetitive behaviour as defined, may end 'spontaneously': that is, as a result neither of general fatigue nor of the performance of a consummatory act (Hinde, 1953; van Iersel, 1953; Kortlandt, 1955, pp. 209, 213; Thorpe, 1956, p. 41). In Kortlandt's picturesque phraseology: 'it looks as if Nature mistrusted appetitive behaviour and therefore has added to it some interim reward'. In other words, this variable activity is sometimes an end in itself, and

the distinction between appetitive and consummatory behaviour cannot be sharply drawn.

Nevertheless, even when random or wandering movements do not end in a specific consummatory act, they can often be shown, or at least inferred, to be instigated by an internal state representing a biological need; in view of this, and of the associations and etymology of the word 'appetitive', it might be thought appropriate to use the term to name those variable movements of an animal which are evoked by an internal state corresponding to a specific primary need (including the need to reproduce). The behaviour would then be defined in terms of physiological state rather than end-result.

The termination of behaviour set in motion by a specific internal state has in the past been ascribed to the *performance* of a stereotyped *activity*, such as drinking, ejaculation or nest-building; but it is now widely conceded that it is not necessarily the performance of an act which is significant, but achieving a particular input to the central nervous system. On this view, the termination of a behaviour sequence is due to the attainment of a consummatory *state*, or afferent pattern (exteroceptive or interoceptive or both) falling on the central nervous system. A consummatory state may result from performing some final act which satisfies a biological need; or it may result merely from performing a preliminary to such an act; or it may arise from the attainment simply of a sensory condition, such as a particular skin temperature in a mammal (Hardy, 1954) or a certain arrangement and number of eggs in contact with a bird's ventral surface (e.g. Moynihan, 1953). The concept of consummatory state has been discussed in some form by Bastock, Morris & Moynihan (1953), by Thorpe (1954*a*; 1956, p. 105), by Kortlandt (1955) and by Barnett (1955). (It is further considered below in VII, 2.) The physiology of such a state must, in the present state of knowledge, often be hypothetical, but there is nothing new in that.

(2) *The variable movements of rats*

Further analysis of 'appetitive' behaviour may be based on an examination of the variable movements of rats about their living space. In wild rats, a generalized movement about a wide area is most readily evoked in two types of situation: first, if a new area becomes accessible, for instance by the removal of a barrier (e.g. Barnett & Spencer, 1951); second, if rats are introduced into an unfamiliar cage or enclosure (Barnett, 1958*a*). When they are in familiar surroundings the behaviour still occurs, though at a lower intensity, and is then most marked when the animals are satiated (Barnett, 1956). As will be seen below (VI, 3), latent learning, or exploratory learning (Thorpe, 1944), is similarly best demonstrated in albino rats when they are not hungry or thirsty or otherwise deprived. If the 'official' definition of 'appetitive behaviour' is adhered to, it is clear that exploratory behaviour released by satiation cannot properly be called appetitive. If it were desired to include 'unmotivated' exploratory behaviour under the heading of appetitive activity, then the definition of 'appetitive' would have to be changed, even if the difficulties outlined in the preceding section were ignored.

It may still be asked to what extent rats display appetitive behaviour *sensu stricto*. That they do so in small-scale movements, for instance in the preliminaries to coitus, is not in doubt, but the answer is less clear when wider-ranging movements are considered. It has long been known that the *activity* of rats is liable to increase in certain physiological states, for instance during oestrus (Richter, 1927). The same effect has recently been demonstrated by Finger & Reid (1952) in rats deprived of food or water. This might seem

to be a measure of appetitive behaviour. But the activity observed, running in a wheel, is not exploratory, but has rather the character of a compulsive motor performance: it may be substitutive, and so analogous to displacement behaviour (Eayrs, 1954). This requires further investigation. In any case, the motion of a wheel tends to stimulate further movement by the rat, once running has begun, and so exaggerates any existing tendency to be active.

It has been doubted whether deprivation by itself increases activity, but this doubt has now been resolved. Jerome, Moody, Connor & Fernandez (1957) have studied the movements of albino rats in a cage with two compartments connected by three doors. To cross from one compartment to another, a rat had to find the one door that was unlocked. The number of crossings was increased by bright light (which was avoided) and by hunger; but even in the absence of any such motivation, many crossings were made. In these experiments, then, an 'appetitive' effect was superimposed on a less labile tendency to explore regardless of specific need.

Campbell & Sheffield (1953) had earlier drawn attention to the importance of environmental change in influencing activity (see also III, 2 below). They studied the 'random activity' of rats in a constant environment, and in their experiments food deprivation alone was followed by little increase. If, however, the deprivation was accompanied by environmental change, there was a great increase. They concluded that 'hunger drive' brings about a lowered threshold of response to change, but is not in itself an internal stimulus to activity. Adlerstein & Fehrer (1955) came to the same conclusion on the basis of experiments with rats in a complex maze. Still more recent work has confirmed that these workers are right in attaching importance to lowered threshold of response to environmental change, but has combined a demonstration of this 'arousal' effect with further proof of the influence of physiological need. Hall (1956) tested rats in activity wheels (*a*) in the dark, and (*b*) while subjected to external stimulation from flashing lights and noises; some of the rats were hungry, some sated. In these conditions, rats did show increased activity resulting from external stimulation, but a still greater increase attributable to hunger. Strong (1957) has confirmed once again the increased activity due to hunger; he has further shown that the activity affected is of the gross locomotor type and not fine, non-locomotor movements. This work emphasizes the importance of the particular method used in measuring 'activity'.

Fehrer (1956), perhaps unusually for a psychologist, has drawn attention to the biological function of exploration. She suggests that, to test the effect of internal state on exploration, it is better to give rats the opportunity to enter a new place from an unfamiliar one, than to transfer them from a cage to a maze. In her experiments, hungry rats showed a greater readiness to leave a familiar place than did sated rats.

Evidently activity in rats is evoked by two types of stimulus situation. First, environmental change, influencing the exteroceptors, increases exploration. Secondly, deprivation (*a*) lowers the threshold of response to external change, (*b*) in itself may increase, not only gross locomotor activity in general, but also exploration proper. Some theoretical implications of this are discussed later (VII, 2).

(3) *The classification of variable movements*

From the preceding survey it follows that there are two ways or dimensions in which the variable movements of an animal might be classified. First, appetitive behaviour

might be distinguished from exploration: in this case the distinction could be based on (hypothetical) internal states, since the term 'appetitive' would be applied only to behaviour 'motivated' by physiological processes related to certain specified biological needs. We have already seen what difficulties arise if this sort of classification is used.

Secondly, there is a possible distinction between (a) the large-scale movements of an animal about its living space, and (b) the minor variable movements which immediately precede some specific act or the attainment of a specific state. For instance, a female rat with young will at intervals *explore* the space constituted by her home range; she will also, when necessary, search around a strayed nestling and retrieve it: the retrieving is a consummatory act, preceded by a variable phase of activity, namely, the approach to the young. There is, however, no clear boundary between the large-scale movements on the one hand and those covering only a short distance on the other.

Neither of these classifications is satisfactory, because each encounters difficulties when the attempt is made to apply it to the observed facts of behaviour. A summary statement of these facts is as follows. Animals perform variable movements covering both large distances and small. Often, these movements end when a particular nervous input (consummatory state) has been achieved (whether by performance of a consummatory act or not). Sometimes, the activity ends without the attainment of any recognizable consummatory state; but in either case it is often possible to assert, either from experimental evidence or by inference, that the activity has been set in motion by a *specific* internal process. Some of these variable movements are exploratory in the sense that they place the animal in contact with many different parts of its environment. All these diverse activities may be labelled 'appetitive'; if this is done, the main function of the label is to distinguish them from those stereotyped patterns of behaviour such as courtship ceremonies, territorial displays and so on which are, by comparison, not variable but fixed (that is, predictable in detail) within each species. As Thorpe (1956) has emphasized, it is in the variable phase of activity that learning principally takes place.

The term 'appetitive behaviour' then comes to mean *any variable behaviour*, and it is necessary to state for each instance to what 'goal object' (if any) and to what internal state (if known) it is related. Sometimes exploratory behaviour is unrelated to any 'goal object', and independent of any internal state other than that which brings about the exploratory behaviour itself. Among rats, this behaviour is observed in its purest form in the laboratory varieties, protected as they are from environmental hazards and selected for tameness (Barnett, 1958*b*). The internal state or processes involved are probably similar to those which instigate play (Lorenz, 1954; Kortlandt, 1955, pp. 225 et seq.; Thorpe, 1956, pp. 84 et seq.) Both exploration of this sort, and play, are released when primary needs are satisfied, and the animal is free to behave in an 'irresponsible' way.

III. STIMULI EVOKING EXPLORATION

(1) *Spontaneous alternation and 'reactive inhibition'*

The preceding section has been concerned with the description of exploratory and similar behaviour, and has made some reference to its internal ('physiological') causes. The next step is to examine in detail the causes of exploratory behaviour, especially the *external* stimuli which influence it. It is convenient to begin with experiments on spontaneous alteration. This phenomenon is observed in maze-running experiments with

equally rewarded or unrewarded alternatives, and most clearly in a maze in which there is only one point at which a choice of routes has to be made. In such a T- or Y-maze, if rats are given two successive opportunities to make this choice, on the second occasion they tend to make the choice not made on the first: if a rat has turned left on the first occasion, it will probably turn right on the second. The same alternation can be observed in more complex mazes.

Montgomery (1951) pointed out the relationship of alternation with exploration: alternation, when the results in terms of reward are equivalent whichever choice of route is taken, is a special case of exploratory behaviour, in that it tends to increase the animal's range of movements. It follows, therefore, that any theoretical interpretation of spontaneous alternation is likely to have a bearing on exploratory behaviour in general.

The most important theoretical discussion until very recently was that of Solomon (1948), who suggested that alternation could be explained in terms of Hull's (1943, 1951) principle of reactive inhibition. This is formally stated as follows (Hull, 1951): 'Whenever a reaction (R) is evoked from an organism there is left an increment of primary negative drive (I_R) which inhibits to a degree according to its magnitude the reaction potential (${}_SE_R$) to that response.' The empirical generalization contained in this theorem is that the performance of an act is followed by a period during which the probability of that act being repeated is reduced. This inhibitory effect dissipates gradually: as an example, Heathers (1940) showed that alternation in a maze decreased proportionally with increase in running time; for instance, with an interval between choices of 15 sec., alternation occurred in 83.5% of trials while, with an interval of 120 sec., the percentage was 65.6.

When a response is rewarded, it tends to be repeated. It was assumed by Hull that this is due to an excitatory potential which persists for much longer than the reactive inhibition. Both excitatory and inhibitory potentials, in this scheme, are theoretical constructs expressing generalizations about behaviour: they are 'intervening variables'; they may, however, be regarded as referring to processes, not yet identified, going on in the central nervous system.

Hull included in his concept of I_R the hypothesis that failure to repeat a response is due to the *performance of the response*. Montgomery (1952*a, b*, 1953*a*) and Glanzer (1953*a, b*) independently published evidence that this hypothesis is incorrect. Glanzer suggested that failure to repeat a response can appropriately be described in terms of *stimulus satiation*: with continuous exposure to an environment, that is, to the same constellation of external stimuli, the animal becomes less active in that environment. This would apply, in Glanzer's view, both to spontaneous alternation and to exploratory behaviour in general. Sutherland (1957) has developed this notion by running rats in modified T-mazes in which the goal was invisible at the choice point. In one type of maze both arms led to the same goal, in the other there were two, dissimilar goal boxes. His experiments confirmed that an important factor in spontaneous alternation is avoidance of stimuli recently experienced; further, this was the case even though the stimuli were not present to the animal at the choice point.

Walker, Dember, Earl & Karoly (1955) have made a very detailed analysis of the causes of spontaneous alternation. They used a + maze which could be rotated, and of which one arm could be blocked to give a T; they showed that in such a maze the factors responsible for alternation were partly the visible difference between the arms of the maze itself (painted black or white) and partly the visible difference between the backgrounds of the

different arms. The effect of the alternation they observed was to vary the *visual stimulus* received by the rat in successive choices, but there was no tendency to vary the *response*, that is, to alternate between turning right and turning left. In this carefully designed study the conclusion again emphasizes the importance of the stimulus pattern, and not the response pattern, in inducing alternation.

In a later study, Walker, Dember, Earl, Fawl & Karoly (1955) showed that it is possible to design an experiment in which alternation is determined partly by the response. They used a T-maze in which the entries to the two goal arms were complicated: each involved twists of the body and changes of direction, and the contortions demanded of the rat were quite different in the two arms. In these conditions, response alternation was nearly as marked as stimulus alternation. The authors suggest that an important factor is the readiness with which two alternatives can be *discriminated*; merely turning left or turning right are not very different, but one set of contortions is readily distinguished from another.

An important question is whether response alternation and stimulus alternation can both be regarded as consequences of a single fundamental process. Dember (1956) exposed rats to a T-maze in which one goal arm was painted white and one black, but he prevented them, by means of glass barriers, from entering either arm on the first run. On a second exposure, both arms were now painted the same colour (either black or white), and either could be entered. Most of the rats explored the *changed* arm. Dember therefore emphasizes the importance of *novelty*. It seems that alternation is essentially an expression of a tendency to behave so that the input to the central nervous system, whether through the exteroceptors or through proprioceptors as in the case of response alternation, is varied. It is as if the animal were continually behaving so as to avoid boredom (cf. Myers & Miller, 1954). This principle probably applies to all types of external stimulation: for example, rats given a choice of three or four foods, of which one is more acceptable than the others, nearly always regularly sample the alternatives; they tend to do this after a period of feeding on the preferred food (Barnett & Spencer, 1953*b*; Barnett, 1956). Butler (1957) finds that even 'auditory exploration' can be a factor in the behaviour of rhesus monkeys.

(2) *Attention to change*

All recent studies, not only of spontaneous alternation but of exploratory behaviour generally, reinforce the conclusion that variation in the stimulus situation is important. It might be thought, however, that general exploratory behaviour is an expression of a tendency to be active. Among those who have shown that this hypothesis is untenable are Berlyne (1950) and Montgomery (1952*b*, 1953*a*, *c*). Glanzer (1953*b*) applied his notion of 'stimulus satiation' to exploratory behaviour as well as to alternation; and Berlyne (1955) used the term 'stimulus hunger' to describe the tendency of rats (and of other species) to investigate unfamiliar objects and situations.

The experiments of Berlyne (1950) showed that rats spend more time exploring new things than things which they have already explored, and that the second time they encounter a new situation they spend less time exploring it than the first. Thompson & Solomon (1954) confirmed Berlyne's findings by showing that rats presented with a new visual pattern explored more than do rats presented with one already experienced. Montgomery (1953*a*) used mazes which were identical apart from being painted black,

grey or white, and found that the amount of exploratory behaviour shown in the second maze entered by a rat was a function of its similarity to the first. In this paper Montgomery speaks of an 'exploratory drive'. In the work so far discussed the three factors of novelty, complexity and spaciousness were not clearly distinguished. Berlyne & Slater (1957) have accordingly done experiments in Y-mazes in which these parameters could be separately studied. They found evidence that all three help to evoke exploration.

It might be thought that the effect of spaciousness reflects a tendency on the part of the rat to 'escape'—a suggestion sometimes made in discussion, even if not in the literature. But, in fact, exploration is typically followed by *return* to the nest or other central point. The notion of escape or freedom for its own sake is an anthropomorphism: as Hediger (1950) has pointed out, one must specify from what the particular individual is escaping (for example, disturbance by men or a hostile companion). Exploratory behaviour does not in itself result in a permanent exchange of one environment for another: it does so only if there is some specific motivation for such a change. When there is no such motivation, exploration merely leads the animal to experience all the accessible parts of a particular range (see also VI, 1 and VI, 3).

Further justification for expressions such as 'stimulus hunger' is found in the observations of Montgomery (1954) and of Myers & Miller (1954), who found that the opportunity to explore an area can act as a reward in learning experiments. Merely presenting novel stimuli, without increasing the space available for exploration, constitutes an effective reward (Chapman & Levy, 1957). Similarly, Dember, Earl & Paradise (1957) find that rats tend to move from conditions of less complex visual stimuli to those in which the stimuli are more complex. The strength of the tendency to explore, even when the animal is in a state of primary need, has been further demonstrated by Zimbardo & Montgomery (1957). In their experiments rats were placed in a 'checkerboard maze' of nine squares in each of which there was a cup which held food or water. In these conditions thirsty rats explored for many minutes before drinking, hungry rats for longer still before eating. These experiments recall the observation of Krechevsky (1937) that rats will choose the longer of two alternative routes to food, if the longer route is varied. This has been confirmed by Hebb & Mahut (1955) and by Havelka (1956).

There is evidence on similar lines for several other species. In rhesus monkeys it had already been shown that the opportunity either to carry out manipulations, or to 'explore' visually, can act as effective rewards in learning situations (Harlow, 1950; Harlow, Harlow & Meyer, 1950; Harlow & McClearn, 1954). Thompson & Heron (1954*a, b*) have shown that, in both rats and dogs, restricted opportunity for exploration in early life is followed later by greater exploratory activity than is shown by control animals. For man, Bexton, Heron & Scott (1954) have studied the effects of sensory deprivation. Reduction of external stimuli to a minimum, in comfortable conditions, led to a demand for stimulation. If this demand were not satisfied, extensive fantasies and even hallucinations were reported. At the end of the experimental period behaviour was for a time considerably disorganized. For our own species, however, it is hardly necessary to provide a formal demonstration of 'stimulus hunger' (cf. Hebb, 1955).

All these observations lead to the conclusion that exploration is a behaviour 'pattern' in its own right. Dember & Earl (1957) suggest that this behaviour belongs to the category of *attention*: this they define as 'any behaviour, motor or perceptual, which has as its end-state contact between the organism and selected portions of its environment'. The

experimental techniques which arouse attention are those which produce change: the change may be in time, as in experiments in which a rat is put in a maze, removed and then returned to it after an alteration has been made in the structure or appearance of the maze; or the change may be spatial, as in a situation in which an animal is faced with a number of separate, unfamiliar objects. But spatial change is not fundamentally different from temporal, since the differences have to be perceived by means of a scanning process which takes time. It follows also that the distinction, referred to above, between complexity and novelty, is of minor importance.

To this may be added that, whether the change is spatial or temporal, there is an 'expectation' or 'set', derived from previous experience, which is compared with the new situation; in the absence of fear, the greater the difference from expectation, the greater is the probability of approach to the new object or situation. The conclusion must be that animals will move about their living space without any impulsion from the 'homeostatic drives' such as hunger, thirst and so forth. On the one hand, novelty tends to impel investigation or, to put the point more non-committally, novelty leads to 'approach behaviour'; from this follows the notion of a 'curiosity drive': novel stimuli have 'reinforcement value' in a learning situation. On the other hand, rats (and other animals) tend to behave in such a way as to put themselves in positions in which they experience new constellations of stimuli (Berlyne, 1950). Activity persists until novelty is exhausted, or until some specific need becomes overwhelmingly strong.

IV. AVOIDANCE BEHAVIOUR

The investigatory activity of rats is so highly developed that in wild rats it is held in check by an opposite tendency, that of avoidance. Chitty & Shorten (1946), Chitty (1954) and Shorten (1954) describe observations on wild rats in the field: the essential feature of their experiments was the placing of an unfamiliar object, such as a box or a pile of food, in a place which the rats were already accustomed to visit; or a familiar object was sometimes put in a new position. They found that such objects were often completely avoided for hours or even days, and that this 'new object reaction' could greatly influence feeding behaviour. This avoidance is in contrast to the response of wild rats on being placed in an entirely unfamiliar cage, or to being suddenly given access to a new area: in both of these situations wild rats, like tame ones, explore extensively without delay, though in a leisurely manner (Barnett & Spencer, 1951; Barnett, 1958*a*). The relationship between exploration and avoidance in wild rats, its effects on feeding behaviour, and the differences between tame and wild rats have been examined in detail by Barnett (1958*b*). In his experiments rats were kept in small cages. Some wild rats avoided unfamiliar objects even though this entailed going without food for one or more days; by contrast, albino and hooded rats investigated new objects at once and continued to feed at their normal daily rate. (Although albino rats do not display 'new object reaction' or neophobia, they can of course be frightened by other types of stimuli: when they are so frightened, their exploratory behaviour is interrupted (Montgomery, 1955; Montgomery & Monkman, 1955).)

A relationship between 'curiosity' and 'fear' has often been asserted (Darwin, 1874, pp. 108 et seq.; McDougall, 1908, pp. 57-8): recently it has been discussed by Berlyne (1951) with reference to rats and man, by Hinde (1954) for the chaffinch (*Fringilla coelebs*)

and by Thorpe (1956, p. 95) in relation to habituation. Dember & Earl (1957) make no distinction between approach and avoidance for the purposes of their discussion: they point out that both involve attention.

In wild rats, as we have seen, investigation dominates in a totally new environment, while avoidance (flight, 'fear') results from a relatively minor change in a familiar constellation. The difference between a wild rat's response to total novelty on the one hand, and to a 'new object' on the other, corresponds to a distinction made by Berlyne (1951). He points out that 'change' or 'novelty' may signify either (a) new in relation to the individual's whole life history, or (b) new in relation to what has been previously experienced. It is the latter which may cause 'surprise', 'fear' and sometimes flight, instead of 'curiosity'. The important factor in the evocation of avoidance behaviour is evidently a contrast between what is expected and what is observed. In one respect the result of avoidance is the same as that of approach: the stimulus, which at first arouses an 'emotion' eventually comes no longer to do so. When the response is investigation, the stimulus gradually becomes ineffective through habituation; when it is flight, by its removal from the perceptual field. Even when flight is the first response, investigation comes later; and if the new object is food it will eventually be eaten. This response to an *unfamiliar* food is wholly different from that to a *distasteful* one: even when hunger forces a rat to eat the latter, no appetite for it develops and, as soon as opportunity offers, another food is chosen (Barnett & Spencer, 1953a).

Hebb (1946) provides an example of 'neophobia', similar to that of rats, in his study of fear and anger in chimpanzees. Of the stimuli which evoke these responses he writes: 'An essential feature of the stimulating conditions is the divergence of the object avoided from a familiar group of objects while still having enough of their properties to fall within the same class.' It is possible that this sort of behaviour is widespread at least among mammals. It is, however, certainly not universal. Southern (1954) has shown that in the house mouse (*Mus musculus*) it is replaced by a mercurial and unpredictable type of behaviour: and Crowcroft (1951) found no 'new object reaction' in shrews (*Sorex araneus* and *S. minutus*).

V. EXPLORATORY BEHAVIOUR IN ONTOGENY

(1) 'Innate' or 'learned'?

Behaviour is often described as either 'innate' ('instinctive') or 'learned', with the implication that the two categories are, on the whole, separate. Some difficulties arising from the concept of instinctive behaviour have already been mentioned (II, 1). The distinction between 'innate' and 'learned' is commonly implied to be a developmental one: while innate behaviour develops (it is supposed) in any environment which allows the animal to survive, learned behaviour depends on the specific conditions acting on the individual, and in particular acting on the animal's nervous system.

In which category, then, should spontaneous alternation, exploration and avoidance appear? In all three, the responses depend on previous experience, that is, on the experience of relationships which can be compared with the new ones; nevertheless, the response to change is made at once, without any gradual learning by practice.

It might be argued that all these instances of response to novelty are examples of insight behaviour. The latter is customarily listed as one of the categories of learned behaviour (e.g. Thorpe, 1956); it is difficult to devise a definition to cover all the various sorts of

behaviour referred to by this term, but Thorpe (1950) suggests 'the sudden production of a new adaptive response not arrived at by...trial and error behaviour'. The word 'random' precedes 'trial and error' in the original passage: it has been omitted here because it is questionable to what extent trial and error behaviour is ever random in the rigorous, statistical sense. In so far as spontaneous alternation is a general phenomenon in the animal kingdom, it must follow that truly random movements do not occur. In the definition as quoted, the word 'adaptive' seems to contain a subjective element, since it appears to suggest that the observer must approve of the response before classing it as 'insight behaviour'. It might be claimed that 'adaptive' means 'favourable to the survival of the individual or the species', but this is not an easy criterion to apply: the survival value of particular acts of behaviour is sometimes difficult to assess.

This comment on the concept and definition of 'insight behaviour' is not intended to disparage the valuable discussion and definitions contained in Thorpe's paper. The object is to discuss difficulties of a sort, already encountered in II, 1, which arise almost inevitably when one attempts formal definitions of terms used to describe complex and diverse activities. The responses to familiar and to novel stimulus situations described here have two things in common with the various sorts of 'insight behaviour': they depend on previous experience, and they are sudden. They differ, however, in taking the form always of approach ('curiosity') or avoidance (flight, 'fear'); the movements are thus always orientated on the new object or situation, and are not related to a 'goal-object' such as food or to a consummatory state such as a full belly. Exploratory behaviour does, however, achieve a 'consummatory situation' in which the whole of an area has been 'investigated' and, as will be seen further in VI, 1, the topography of it has been learnt; but this does not apply to avoidance.

It seems doubtful whether any purpose could be served by calling exploration or avoidance insight behaviour, or indeed by putting these types of response in any of the standard categories of learned behaviour. (It will be seen, however, in VI, 3, that insight learning may *accompany* exploration.) Hebb (1949, p. 245) considers that avoidance behaviour can properly be labelled neither 'innate' nor 'learned'. The same applies to exploration (and possibly to many other types of behaviour). Perhaps a new terminology is needed; but, whatever vocabulary is used, it seems that there is at present no justification for grouping all kinds of response to novelty under one head.

(2) *Learning to learn*

Whether response to novelty is called 'innate' or 'learned' or neither, it is certain that it occurs in some form very early in post-natal development. Rigorous experiments have not been done on the exploratory behaviour of infant rats but, as Small (1899) first pointed out, rats move about a good deal soon after birth; some of this movement seems to have nothing to do with seeking food or warmth, though it may be related to specific tactile stimuli. Detailed study is needed on this point. Manipulatory behaviour has been observed in wild rat nestlings: at the age of 13 days, before the eyes were open, they have been seen to take fragments of wood in both hands and, in a clumsy manner, convey them to the mouth. (Harlow *et al.* (1956) have similarly observed 'self-sustaining' manipulatory behaviour in the infant rhesus monkey.) Further, as soon as the eyes open, at about 17 days, general exploration of the environment begins, although this involves leaving the nest and the mother; within a few days the topography of an area around the

nest has been learnt, and this area is repeatedly re-explored during play (unpublished observations).

Hebb (1949, p. 298) has discussed the difference between early and late learning in mammals, and has emphasized the peculiar importance of the former. He gives evidence that rats allowed a richer experience of diverse environmental conditions early in life display greater 'intelligence' as adults: that is, they are better able to profit from new experiences than are rats brought up in a more restricted environment. The existence of this 'deutero-learning' (Bateson, 1942) in rats has now been confirmed in a number of studies. Bingham & Griffiths (1952) and Forgays & Forgays (1952) repeated Hebb's observations with a more rigorous technique, and Hymovitch (1952) showed that diversity of experience later in life has relatively little effect on learning ability; he also emphasized the importance of perceptual, as distinct from motor, learning. Gibson & Walk (1956) brought up rats from birth in cages with white walls bearing black circles and triangles. These rats learned to discriminate between a circle and a triangle sooner and with fewer errors than control rats which had had an undecorated home.

Forgus (1954) found that the effects of this early perceptual learning are far from simple: giving rats in early life a complex environment, whether it could be explored or only seen, resulted in greater activity, less 'emotionality' and more variability of behaviour in an unfamiliar situation, than was shown by controls; discrimination of visual forms and spatial problem-solving were also superior in the experimental animals. In later experiments Forgus (1955) showed that early motor experience too can influence learning ability. One group of hooded rats was given much early visual and motor experience in a large and complex environment; a second group had similar visual experience but much less opportunity for motor activity. When the rats were tested, as adults, in a maze, the second group did the better of the two if the lights were kept on, while the first group did better in darkness, that is, in conditions in which learning had evidently to be based on kinaesthesia. This is a further example of the fact, already exemplified from studies of spontaneous alternation (III, 1), that learning can be related to any aspect of the input to the central nervous system, exteroceptive or proprioceptive. The work of Forgus (1956), like that of Hymovitch already quoted, also supports Hebb's hypothesis on the special importance of early deutero-learning. Forgus found that form discrimination and form generalization in learning experiments was enhanced by giving hooded rats experience of the forms used from the time the eyes opened, at 16 days, to the age of 41 days; and that similar exposure from 41 to 66 days was less effective.

The very extensive work on rats is paralleled by a smaller number of studies on other species. Thompson & Heron (1954*a, b*) have found similar effects of early experience on the 'intelligence' of dogs. Melzack & Thompson (1956) find that early restriction interferes with the normal development of social responses in dogs. In chimpanzees Nissen, Chow & Semmes (1951) have demonstrated the damage done to perceptual function by reducing tactual and kinaesthetic stimulation early in life. There can be no doubt that for mammals at least early deutero-learning is of prime importance.

It will be shown below (VI, 1) that exploratory behaviour is of the greatest importance in the process by which an individual adapts itself to its own particular environment. The evidence given in the preceding paragraphs suggests that, in mammals at least, diverse experience in early life, both sensory and motor, is also necessary for the full development of function in the central nervous system. It follows from this that there is a 'biological

need' for such experience: that is, early exploratory behaviour, by making possible a variety of sensory and motor experience, has survival value. If this is so, it is not surprising that such behaviour is found to be carried out 'for its own sake', and not merely in relation to satisfying one of the conventionally accepted homeostatic needs. The evidence for exploration as a 'behaviour pattern' in its own right, and for 'stimulus hunger', given in III, 1 and III, 2 above, might well have been sought as a result of inference from biological principles.

VI. ECOLOGY OF EXPLORATION AND AVOIDANCE

(1) *Exploratory behaviour*

The discussion so far has been principally in terms of the stimulus conditions which evoke investigation or avoidance, with some reference to the largely unexplored field of the internal states concerned. Something further may now be said about the biological function, or survival value, of these types of behaviour. Wild rats, once they leave the nest, are subject to predation, and their principal means of avoiding predators are the use of pathways under cover, and flight to a burrow or other place of concealment. This behaviour depends on previous experience of the topography of their living space. Given this, they can run from any one point to any other, by the shortest route and in the least possible time. The behaviour of a rat violently disturbed in strange surroundings is quite different: it leaps and runs in an erratic and undirected manner, and often passes covered places of refuge to which it would resort at once if it were familiar with the conditions (unpublished observations). A rat in these circumstances does not pause to reflect. The effect of this is illustrated by the work of Elton (1953), who has brought evidence of the greater vulnerability of rats in a strange area to predation by cats.

Exploration does not, however, lead only to learning where cover and nest sites are: it is accompanied by sampling of all the edible or potable materials encountered (Barnett & Spencer, 1953*b*); and so a rat is regularly informed of accessible sources of food and water (Barnett, 1956). A further consequence of general movement about the living space is that odour trails are left on the routes regularly used, and these are detected by other rats (Reiff, 1952, 1953).

(2) *Avoidance behaviour*

If wild rats behaved as tame ones do towards unfamiliar objects, their compulsive exploratory and sampling behaviour would quickly lead them into traps or result in the ingestion of poison bait. 'New object reaction' or neophobia, displayed in a highly developed form by wild rats, protects them from the consequences of 'curiosity'; and the studies, already quoted, of Chitty & Shorten (1946), Chitty (1954) and Shorten (1954) have shown how effective this behaviour is in preserving rats from human efforts to kill them. It seems likely that the extent to which wild rats commensal with man display avoidance behaviour is a consequence of selection over the 7000 years during which civilization, and therefore stores of human food, have existed. (More intensive selection *against* wildness and neophobia in laboratory rats has been completely successful in less than a century.)

The neophobia of wild rats is not by itself sufficient protection against poisoned food. It is combined with a capacity to learn to refuse toxic mixtures (Rzoska, 1953, 1954). This parallels the well-known ability to select, in certain instances, the nutritionally superior of two foods: such dietary self-selection (reviewed by Munn, 1950) is in part a result of

associative learning. We have seen that a wild rat regularly samples everything edible within range, although unfamiliar foods are at first avoided and then provisionally sampled only in very small amounts. This combination of exploring and avoidance with learning is elegantly adapted to giving a rat the maximum of information about the resources and dangers of its environment, in the safest possible way (Barnett, 1956).

(3) *Exploratory (latent) learning*

Wilkinson (1952) has given a mathematical analysis of the effects of strictly random exploration; his concern was with the 'navigation' of birds, but the analysis has a quite general application, and demonstrates that random searching is a remarkably effective method of reaching a specific goal. But the studies of spontaneous alternation show that exploratory behaviour is not random: on the contrary, areas which have just been explored tend to be avoided; this must make exploration correspondingly more efficient, either in leading the animal to a particular goal such as food, or in making it generally familiar with a large area. Further, most of the movements of animals are directional, for instance, to or away from light (phototaxis). Rats are specifically influenced by tactile stimuli, as well as light, and tend to move in contact with vertical surfaces. This further reduces the randomness of their movements. It also tends to keep them under cover. This tendency is, however, not strong enough to prevent them from exploring, eventually, all parts of their home range.

Whether random or not, exploration would be largely ineffectual without a concomitant learning process. Accordingly, in the preceding sections it has been taken for granted that exploratory behaviour is accompanied by topographical learning. Certainly, it is obvious that wild animals rapidly become acquainted with the topography of a newly colonized area. But the way in which such learning is related to exploration has been rigorously established in laboratory studies of tame rats.

Just as exploratory behaviour is performed 'without motivation', so apparently unmotivated learning takes place while it is being carried out. This *exploratory (latent) learning* has been defined as 'the association of indifferent stimuli or situations without patent reward' (quoted by Thorpe, 1956, p. 91). This subject has been so fully reviewed by Munn (1950, pp. 315 et seq. and 401 et seq.), by Thistlethwaite (1951) and Thorpe (1956), that only a brief account is needed here.

The essential features of this behaviour were first made clear by Blodgett (1929). If rats are given experience of a maze, but are not rewarded on reaching the goal point, they explore the maze. If, later, they are run in the same maze in the usual way, with a reward of food at the goal point, they learn the maze more quickly than controls which have had no unrewarded experience of the maze: they make fewer errors and their running time is shorter.

This apparently 'unrewarded' learning was received with scepticism by some psychologists who were wedded to the 'law of reinforcement', according to which learning takes place only when the animal is 'motivated'. Hull (1943), in attempting to make the concept of motivation more concrete, equated it with a state of 'primary need', but Hebb (1949, pp. 176 et seq.) and Hebb & Mahut (1955) have effectively criticized this formulation. As far as the specific case of exploratory learning is concerned, many workers have confirmed Blodgett's observations (see the reviews already quoted). In some experiments, however, exploratory learning did not take place when it might have been expected.

These were studies in which the rats were not satiated during their preliminary experiences of the maze, but were in a state of 'irrelevant drive'. For example, Spence & Lippitt (1946) trained rats to run a Y-maze while thirsty; one path led always to water, the other to food for half the rats and to nothing for the other half; all the rats were able to explore the maze. When learning was established, the rats were run while hungry but not thirsty; and food was put in the, formerly empty, goal box of the control group. In these conditions, no difference was found between the two groups in speed of learning to go to food, although it might be expected that those rats which had had experience of food in the goal box when not hungry would have learned the task sooner. The evidence from such experiments suggests that generalized exploratory learning takes place only in the *absence* of special need, for example for food or water: when an animal is severely deprived, it learns only those responses which lead to the relief of its particular need.

It was shown in II, 3 that the variable movements of rats (appetitive behaviour in the general sense) may be related to a specific need or may be performed 'for their own sake'. Similarly, as the preceding paragraphs show, the learning that accompanies movement about the living space is of two kinds. First, there is the learning which results from encountering a specific goal object (food, water, nest site, nest materials or other rats); the same type of learning occurs if a noxious stimulus, such as harmful food, is experienced. Secondly, the animal is all the time liable to be influenced by the non-specific stimuli of its environment. Exploratory behaviour occurs at all ages in both tame and wild rats (Barnett, 1956), and it must be supposed that the central nervous system, especially the cerebral cortex, is being continually modified to some degree by the afferent impulses reaching it; this effect is evidently most generalized when the animal is not in a state of primary need. It is comparable to, but more specific than, the deutero-learning referred to in V, 2.

VII. EXPLORATION AND THEORIES OF BEHAVIOUR

(1) *Learning*

The study of exploratory behaviour has influenced the ways in which certain aspects of behaviour are interpreted. First, it has been used to throw light on the nature of complex learned behaviour.

The topographical learning that accompanies variable movements about the living space, whether related to some specific state of need or not, is exceedingly complex. This topic (reviewed by Thorpe, 1956, pp. 91 et seq., 109 et seq.) is still controversial. One of the problems is the extent to which this behaviour can be regarded as the learning of a sequence of motor acts which lead the animal through a succession of positions from the starting point to the goal. An interpretation in these terms is sometimes called a response theory, or *SR* theory, of learning. It stems from the notion, long discarded by most students of animal behaviour, that behaviour can be adequately described as if it consisted entirely of 'reflexes' and 'conditional reflexes'. It has been known for more than thirty years that no such theory can account for all the behaviour shown in mazes or other situations requiring problem-solving, because animals can use different series of movements, on different occasions after a task has been learnt, to solve a problem. The early work on this subject was reviewed with admirable lucidity by Lashley (1929, 1934). This behaviour comes within the category of insight behaviour as defined in V, 1. Many experiments have been carried out, since Lashley wrote, confirming that rats can profit

from separate previous experiences in taking the shortest route to a goal, and that in doing so they may follow a path which they have never employed before. Tolman (1948), in a review of this work, writes that it is as if 'in the course of learning something like a field map of the environment gets established in the rat's brain' (see also Caldwell & Jones, 1954). Tolman uses the term 'expectancy' to describe the state of a rat when it is behaving in this way.

Very recently, Birch & Korn (1958) have done further experiments to test Tolman's idea of 'cognitive maps', and have subjected the various theoretical positions in this area to critical scrutiny. They point out that, even though simple stimulus-response theories of topographical learning are inadequate, it does not follow from this that the behaviour can be explained only by a 'cognitive' or 'expectancy' theory. Their experiments, carried out in apparatus similar to that used by Tolman and his colleagues, suggest that, when a rat seems to be behaving as if it possessed a cognitive map of a maze, it is in fact responding to particular stimuli, such as differences of illumination, which are associated with food; they further find that part of the series of responses depends on kinaesthetic cues. Their position then includes a 'response' element, in that proprioceptive stimuli are held to guide the rat in some of its movements. But other movements may be directed towards the goal, as a result of previous learning of the position of the goal in relation to sources of stimulation; and these movements are not mere repetitions of those previously learned in a different situation.

The discussion is unlikely to stand still at this point. So far, it has effectively shown that very simple theories of topographical learning are untenable, and it has at least displayed some of the difficulties of constructing a satisfactory one.

(2) *Motivation*

The second theoretical aspect of the study of exploratory behaviour concerns the internal states which make an animal active: that is, it concerns motivation. Here it is desirable to refer again to the two distinct ways in which behaviour may be 'explained'. First, an activity may be said to have survival value for the individual or species in which it occurs, and so be explained by the action of natural selection; the causes of the behaviour, implied in any such statement, are then events in the comparatively remote past which have acted in the first place on ancestors of the animals observed. Secondly, an activity may be explained as being due to stimuli influencing the animal at the time, or at least during its own life history; or as due to internal processes, for instance in the nervous system.

The first type of explanation is exemplified in VI, 1 to VI, 3: exploratory and avoidance behaviour, and the accompanying learning, have obvious survival value. Many of the easily observed types of behaviour confer some evident advantage on the individual or on the species. This has led to the notion of the homeostatic function of behaviour: an animal's activities are looked upon as all tending to fulfill its primary needs (including those connected with reproduction). Hence, the internal state or motivation responsible for specific activity has been equated with a state of specific need, notably by Hull (1943). However, the fact that behaviour is a product of natural selection, and tends to ensure survival, does not necessarily imply that particular activities are always a direct consequence of the needs they help to satisfy. To say that they are, is to confuse the two types of cause defined above. A kind of behaviour may tend to satisfy a need, and yet be per-

formed regardless of the presence or absence of the need: it may be evoked by internal processes which are independent of such states as hunger or thirst, and by external stimuli which are neutral as far as survival is concerned.

This is the case with exploratory behaviour. Exploration therefore obviously demands a theory of motivation more subtle and complex than those which have prevailed hitherto. It has been shown above (VII, 1) that stimulus-response theories are inadequate to account for exploratory learning. Such theories, based on a misleadingly simple account of conditional reflexes, have also been unable to cope with the obvious facts of motivation, such as the 'spontaneous' fluctuations in an animal's response to a given stimulus or situation. Accordingly, emphasis has come to be laid on autonomous changes in the central nervous system (e.g. Young, 1938; Hebb, 1949).

It is now evident that exploratory behaviour and learning depend on yet a further feature of the total situation, namely, non-specific changes in the stimulus pattern falling on the sense organs. By 'non-specific changes' is meant those which do not necessarily evoke or demand any special consummatory response. General exteroceptive stimulation has long been thought to be important in maintaining central nervous activity in, for example, insects (Wolsky, 1933). The work on the sense organs of invertebrates, with which Wolsky was chiefly concerned, has however not demonstrated any special significance in a *varying* level of stimulation. Nevertheless, Darchen (1952, 1955) has shown that cockroaches (*Blattella germanica*) display exploratory behaviour like that of rats; and it may be that *Stimulationsorgane* are of importance as channels for external stimulation which is needed to maintain this behaviour. This requires investigation.

In mammals such as rats or monkeys the change in stimulation which evokes exploration may result from the presence of objects which are themselves changing; or it may be due to the scanning, by the animal, of an environment containing a variety of objects. This variety both stimulates activity and satisfies a need. In this second quality, though not in other respects, the condition of being stimulated resembles a consummatory state. In II, 1 above, 'consummatory state' was by implication defined as an afferent pattern, impinging on the central nervous system, which reduces or ends an activity. But an important feature of a consummatory state is that it has reward value: an animal tends to learn to achieve it. This is the case too with variety of stimulation (III, 2): the opportunity to experience sensory change alone can act as an incentive to learn some task; but, unlike a consummatory state, it leads to greater activity until the novelty wears off.

Hebb (1955) accordingly distinguishes two types of effect resulting from a sensory event. First, there is the *cue function*, which is the familiar one of guiding behaviour, that is, of determining that a particular behaviour pattern will be performed and orientated in an appropriate direction. Secondly, there is the *arousal function*. The importance of this second function is attested by the evidence already given in the present review. Hebb suggests that the state of arousal, which depends on varying stimulation, is synonymous with a 'general drive state', and that this state is maintained in mammals by the reticular formation of the brain stem: on this view, 'the conception of drive assumes anatomical and physiological identity'. The internal springs of activity which we call motivation are consequently dependent to an important extent on the changing stimulation of the exteroceptors.

VIII. SUMMARY

Many animals have a general tendency to explore their surroundings, that is, to approach and to enter every accessible place within a considerable area around a nest or resting place. This behaviour has been especially well studied in rats (*Rattus norvegicus*), both tame and wild. Exploratory behaviour is one category of the *variable* movements of animals. Often, such movements end when a specific (consummatory) state has been achieved (whether by performance of a consummatory act or not). Sometimes the variable activity ends without attainment of any recognizable consummatory state. In either case, it is often possible to assert, or to infer, that the activity has been *evoked* by a specific internal state (such as hunger). All these diverse activities may be labelled 'appetitive'; this term then distinguishes variable movements from stereotyped action patterns such as courtship ceremonies; the latter are, by comparison, not variable but fixed (predictable in detail) within each species.

The definition of 'appetitive behaviour' thus becomes 'any variable behaviour'. Consequently, it is necessary to state for each instance to what 'goal object' (if any) and to what internal state (if known) it is related. Some exploratory behaviour is, however, unrelated to any goal, and independent of any special internal state apart from that which brings about the exploration itself.

Exploratory behaviour in satiated rats provides a clear example of variable movements unrelated to specific need. It is observed most readily in the laboratory varieties, but is highly developed also in wild rats. It is not a spontaneous motor activity, but a result of a general tendency to behave so as to vary the stimulus situation experienced by the animal from moment to moment, and to avoid what has just been experienced. Other species, from cockroaches to monkeys, behave in a similar way.

'Spontaneous alternation' is a special case of exploratory behaviour. It is not, as has been suggested, a result of inhibition of the motor *response* which has just been performed: in most experimental situations what is avoided is repetition of visual experiences, but it is possible to devise experiments in which response alternation occurs. All experimental data on spontaneous alternation may be subsumed under the heading of '*stimulus satiation*'. Similarly, all exploratory behaviour may be interpreted in terms of 'stimulus hunger'. More generally, it may be categorized as *attention to change*. However, *activity* may be increased, not only by changes in the stimulus situation, but also by deprivation, for instance of food: this (a) lowers the threshold of response to external change, (b) may directly increase activity.

Wild rats given access to an unfamiliar area explore it. They also regularly re-explore their home range. But movement into a familiar area is inhibited by the presence of an unfamiliar object or arrangement. This new object reaction, or neophobia, is not found in tame rats. Its biological value is to protect rats from danger. Similar responses occur in some other species.

Exploratory behaviour begins in early infancy but, having no specific pattern, it differs greatly from behaviour of the kinds commonly called 'innate'. It depends on learning, since it is a consequence of comparing a stimulus pattern with one previously experienced; but it does not fall into any of the conventional classes of learned behaviour. Like avoidance behaviour, it exemplifies the inadequacy of the division of behaviour into 'innate' and 'learned'.

Visual and motor exploration of a complex habitat during early life is necessary for the full development of a mammal's ability to solve problems. Early experience is accompanied by a process of 'deutero-learning', or learning to learn, which occurs little or not at all in the adult.

The survival value of exploration lies not only in the consequent deutero-learning, but also in the learning and relearning of the resources of the animal's environment which accompanies it. In rats this learning, combined with the avoidance of new objects, gives the maximum of information about the environment, in the safest possible way. This exploratory, or latent, learning is 'unrewarded', and its effects become evident only when the animal is later faced with a specific problem. It is a highly complex form of learning, and gives rise to a form of insight behaviour, since it enables an animal to make use of two or more previous experiences to perform a new action.

The study of exploratory behaviour has thrown new light on 'motivation', since it has shown that exteroceptive stimulation has a necessary arousal function which influences the animal's general level of activity.

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THE ATTENTION-GETTING VALUE OF STRUCTURAL CHANGE

By ROBERT T. GREEN

University College London

The structure of a list of items in a serial learning task seems to have a reliable influence upon the probability of recalling a particular item. The results pose serious difficulties for either the gestalt or the usual behaviouristic serial interference interpretations. In particular, it would appear that earlier experiments which were explained in terms of Von Restorff's 'isolation' effect have overlooked and confounded the variable of structural change.

I. INTRODUCTION

Earlier experiments by Saul & Osgood (1950) and Green (1956, 1958) demonstrated that isolation in a temporal series has no effect on recall probabilities. Other published data seeming to support the isolation hypothesis are demonstrably contaminated by factors such as serial interference—a factor used by Gibson (1940) to generate Von Restorff's (1933) results. More recent work, not using paired associates, demands an explanation in terms of either pro-active interference or the attention-getting properties of structural change within the series. Until now it has not been possible to choose between these two hypotheses because an item of a new type has always been used to introduce the structural change, and has therefore not been subject to pro-active interference. In the present study an attempt is made to find a crucial experiment to discriminate between these two hypotheses and to demonstrate that the effectiveness of structural change as a factor favouring recall is due to its attention-getting properties.

II. PROCEDURE

In order to resolve the confusion between pro-active interference and structural change it was decided to make *repetition* of a critical item the structural change. Although the subjects were instructed to learn a letter sequence, they were, in fact, tested on recall of the colour of a critical item. This incidental learning design was necessitated by the fact that repetition of the critical item must favour its recall as a letter. Two groups were used, and reference to the structure of the two corresponding lists in Table 1 will help to make the experimental design clearer. The lists are identical except that for the beta group the blue

Table 1. *Structure of lists*

	List alpha		List beta	
Serial position	1	<i>Q</i>	Red	<i>Q</i>
	2	<i>K</i>	Green	<i>K</i>
	3	<i>S</i>	Yellow	<i>S</i>
	4	<i>G</i>	Black	<i>G</i>
	5	<i>A</i>	Green	<i>A</i>
	6	<i>B</i>	Blue	<i>H</i>
	7	<i>J</i>	Red	<i>J</i>
	8	<i>N</i>	Black	<i>N</i>
	9	<i>H</i>	Yellow	<i>H</i> — Critical item
	10	<i>U</i>	Brown	<i>U</i>
	11	<i>M</i>	Orange	<i>M</i>
	12	<i>H</i>	Blue	<i>B</i>
	13	<i>T</i>	Mauve	<i>T</i>
	14	<i>D</i>	Orange	<i>D</i>
	15	<i>L</i>	Brown	<i>L</i>
	16	<i>P</i>	Mauve	<i>P</i>

letter *H* is exchanged in serial position with letter *B* of the same colour. In both lists the critical item is the yellow letter *H* in position 9. For the alpha group this is the first letter *H*, while for the beta group it is the second. According to our hypothesis the second letter *H* has attention-getting value because repetition in these series is a structural change. So it is predicted that members of the beta group will pay more attention to the critical item than those of the alpha group. Because of the greater attention accorded to this item they are more likely to learn its colour quite incidentally. Hence it follows that the probability of recalling the colour of the critical item should be greater under beta conditions.

Instructions to the subjects followed closely those reported in the earlier paper. No mention was made of the colour variable, the subjects being 'set' to learn letters. Each item was presented for 3 sec., followed immediately by the next, by means of a film-strip projector behind a translucent 18 inch square screen in a semi-darkened room. Immediately after the single learning trial the following instructions were read to the subjects.

'The letter *H* occurred twice in that series. Write down the colours of both the first and second letters *H* in the correct order. That is, write down the colour of the first letter *H*, labelling it 'first', and then directly below write the colour of the second letter *H*, labelling it 'second'. If you are unable to remember one or the other, simply put a cross in the appropriate position. If you can recall neither, put two crosses.' (20 sec. pause for writing.) 'When you have done this, indicate how sure you are of either judgement in the following manner. If you are certain to the extent that you would be prepared to swear to it in a court of law put a letter *C* for certain against the colour you are sure of. If you are fairly sure, but not perfectly sure put the letters *FS* against the appropriate colour. If you are quite doubtful about one or the other colour put the letter *D* beside it.'

The subjects were young adults, nearly all males, attending a Technical College. The use of a three-point scale was intended to refine the results, it being thought that not only should a greater proportion of the beta group recall the colour correctly, but that they should also be more sure of their choice. By asking for recall of both the letters *H* it was possible to see to what extent differences might be due to the subjects allocating the correct colours to the wrong position. The colours of yellow and blue were chosen to take advantage of the low incidence of tritanopia among the general population. No tests for colour defects were made, a haphazard allocation of subjects to the two groups being left to distribute what small effects might exist.

III. RESULTS AND DISCUSSION

The main findings are set out in Table 2. For the alpha group the critical data are the yellow responses for the first letter *H*, and for the beta group the yellow responses for the second letter *H*. These totals, ignoring the rating scale, yield recall probabilities of 0.3

Table 2. Analysis of recall frequencies

First <i>H</i>					Second <i>H</i>					Total subjects	
List alpha	6	10	10	2	Y	5	7	2	—	90	
	2	3	2	—	B	7	7	3	—		
	2	14	24	15	O	10	12	12	25		
	C	FS	D	O	C	FS	D	O			
List beta	5	8	8	—	Y	19	13	3	2	76	
	—	—	6	—	B	—	4	1	1		
	3	19	11	16	O	2	8	11	12		

Y = yellow; B = blue; O = other or unclassified; C = certain; FS = fairly sure; D = doubtful.

and 0.5, and the frequency analysis leads to $\chi^2=5.35$, $p<0.03$ (two-tail), as shown in Table 3.

This result, taken in conjunction with that reported by Green (1958), flatly contradicts both the gestalt and serial interference predictions. According to Von Restorff, the two *H*

engrams should agglutinate; but we have already shown that succeeding items do not affect the recall of a similar earlier item, and we are left with the fact that under these conditions recall is *facilitated* by a similar preceding item. In previous experiments it has been possible to interpret the results in terms of pro-active interference, but, in this case, the alternative behaviouristic account is equally lost. We are therefore driven to conclude that the relevant variable is structural change.

Table 3. *Comparison of recall frequencies*

		R	F	
Group {	Alpha	28	62	90
	Beta	37	39	76
		65	101	166

R = No. subjects recalling colour correctly; F = No. of subjects forgetting.

$$\chi^2 = 5.35.$$

d.f. = 1; $p < 0.03$ (two-tail).

Some additional interest attaches to the ratings of subjective conviction. If we draw our comparison between those who were correct *and* certain of it, 6 subjects in the alpha group and 19 in the beta group, instead of all those correct with varying degrees of conviction, the difference between the groups becomes far more distinct. $\chi^2 = 10.76$; d.f. = 1; $p < 0.005$. A sidelight is also thrown on the question of the reliability of testimony. Of the 61 ratings of certainty only 32 were in fact justified. Nor was this entirely a matter of confusing the first and second letters *H*. Of the 29 'certain' and wrong answers only 12 came under the heading of a simple inversion. Breaking down the 31 ratings of certainty for the critical item we find that for the alpha group, where it was the first letter *H*, the probability of being right when certain is 0.6 (6 out of 10) while for the beta group $p = 0.9$ (19 out of 21). So if the event has mild attention-getting value then the testimony would seem to be more reliable ($p = 0.06$) two-tail. It would not be wise to assume that the relationship is a simple one. Where the event has attention-getting value because of its high emotional content (such as in the case of a witness to a crime), an entirely different picture might emerge.

The data invite one further observation. Contrary to what has been reported in earlier studies by Blackburn (1945) and Munsterberg (1909), but in agreement with the studies surveyed by Whipple (1909), there appears to be a clear-cut relation between the degree of confidence expressed and the probability of being right. For those who were 'certain' the probability of being correct is 0.525 (32 out of 61). For those who were 'fairly sure' $p = 0.286$ (30 out of 105), and for the 'doubtful' replies, $p = 0.236$ (22 out of 93). Testing this against the null hypothesis, $\chi^2 = 14.6$; d.f. = 2; $p < 0.001$. In fact our confidence level is even better when we consider that this value of χ^2 takes no note of the consistent direction of the trend. Blackburn's apparently anomalous result may be simply a function of delay between observing and recalling (his subjects were asked for a report some 2 weeks after the event). Another possibly important variable is the subject's attitude. If the event is staged and the subject does not realize that a report will be required, as with Blackburn's subject, the method of attending may be very different from that adopted in a straightforward memory experiment.

Since the present paper went to press, Newman & Saltz (1958) have confirmed that intra-list interference due to stimulus and response generalization cannot account for isolation effects.

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THE INFLUENCE OF THE ALBEDO AND COMPLEXITY OF STIMULI ON VISUAL FIXATION IN THE HUMAN INFANT

By D. E. BERLYNE

*Centre for Advanced Study in the Behavioural Sciences**

I. INTRODUCTION

For several weeks after birth the human infant divides his time between sleeping, crying and feeding. But, before long, more and more of his time begins to be taken up by a fourth activity, namely, looking. It is clear that his eyes do not wander about the visual field purely at random; his gaze dwells on an object and then shifts to another. Some of the properties that favour fixation of particular objects are fairly obvious and have been reported in the literature—brightness and movement (Hall, 1903; Piaget, 1936), colour (Valentine, 1914; Staples, 1932) and secondary reward value (possessed by human faces and feeding bottles). These variables hardly suffice to account for the direction of all the baby's visual fixations during his first year. But, apart from them, the observing behaviour of the human infant seems to have received little study, especially in view of the time and eagerness that he devotes to it. The experiment to be reported tested the effects of two variables, albedo and complexity. Various factors that can be grouped together under the word 'complexity' have turned out to be important determinants of exploratory behaviour in animals (see Berlyne & Slater, 1957) and of orienting responses in human adults (Berlyne, 1957*a*, 1958). They bear interesting relations to variables figuring in information theory and in the theory of conflict (Berlyne, 1957*b*).

II. THE EXPERIMENT

Stimulus material (see Fig. 1). Four series of three rectangular cards were prepared. Series A consisted of plain black, grey, and white rectangles; series B, C and D each contained three figures with different degrees of complexity, but all made up of equal areas of black and white. Card B3 bore a pattern, kindly supplied by Dr F. Attneave (see Attneave, 1954, p. 188), of black and white dots, randomly distributed. Each pattern measured 6×4.5 in. and was surrounded by a $\frac{1}{2}$ in. grey margin. A paired-comparison technique was used: the three cards in each series were presented two at a time side by side, each of the three pairs occurring once in one spatial arrangement and once in the other. There were thus twenty-four trials altogether.

The twenty-four pairs were arranged in a randomized sequence such that each successive set of four pairs contained one from each of the four series of cards. This sequence was used with half of the infants, while the other half had a sequence formed by reversing the temporal order and interchanging left and right in the first sequence.

Subjects. The fourteen subjects ranged in age from 3 to 9 months. Three were adopted babies, while the remainder were children of students and professional people.

Procedure. It was essential for the experimenter who presented the cards to the infants and observed their eye movements not to know which cards were present in any particular trial. Otherwise, there would have been a possibility of introducing constant errors, either in the manner of holding the cards or in the observation of the direction of fixation. For this reason, there were two experimenters who alternated between two roles.

* Now at the University of California, Berkeley. The experiment was carried out and the paper written while the author was on leave of absence from the University of Aberdeen.

The subject was propped up in a sitting position or lay supine, according to his stage of development. For each trial the first experimenter selected the appropriate pair of cards and clipped them side by side and 6 in. apart on a 9 × 16 in. board. He then handed the board to the second experimenter without allowing him to see the cards. The second experimenter waited until the subject's gaze, spontaneously or by contrivance, was central, whereupon he brought the cards into the infant's field of vision from above, holding the board about 18 in. away. He called out 'right' or 'left' according to the card that the infant *first* fixated, and the first experimenter recorded the result. On one or two occasions, it was impossible to decide which card was fixated first; when this happened, the same pair was presented again later in the sequence.

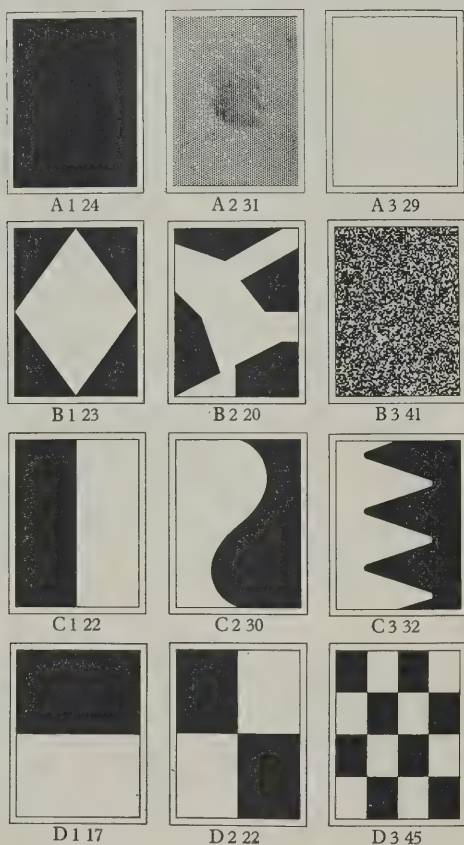


Fig. 1. The stimulus material.

III. RESULTS

The number of times (out of fifty-six presentations) that each pattern was fixated first is shown beneath it in Fig. 1. The results for each series were subjected to two statistical treatments. First, the statistic S (Bradley, 1955, p. 460), which has the χ^2 distribution with 3 D.F., was computed, to ascertain whether there were any significant preferences among the patterns in the series. The relevant data are shown in Table 1, where it will be seen that S is significant for series B and D but not for series A and C. Secondly, the sign test was applied to each paired comparison separately to find out which individual patterns were actually preferred to which. As Table 1 makes clear, patterns B3 and D3

Table 1. *First fixations*

Patterns Series	...	1 and 2	1 and 3	2 and 3	$S (= \chi^2, 3 \text{ D.F.})$
A		12:16 N.S.	12:16 N.S.	15:13 N.S.	1.29 N.S.
B		16:12 N.S.	7:21 $P < 0.05$	8:20 $P < 0.05$	12.71 $P < 0.01$
C		11:17 N.S.	11:17 N.S.	13:15 N.S.	2.71 N.S.
D		11:17 N.S.	6:22 $P < 0.01$	5:23 $P < 0.01$	22.00 $P < 0.01$

were significantly preferred to other patterns in the same series, but no other preferences emerged. The findings can thus be summarized as follows. (1) Differences in albedo did not affect the direction of the first fixation. (2) Two patterns, B3 and D3, were more likely than others to attract first fixations.

Of these conclusions, the first recalls an unpublished experiment by the writer in which rats were confronted with black and white rectangles and approached both colours equally often. Similarly, Montgomery (1953) found no differences among the amounts of exploratory activity by rats in white, grey and black alleys. Gross differences in intensity of light, on the other hand, appear to influence the responses of rats and the fixations of human infants.

It is noteworthy that the two patterns in the complexity series that were especially attractive were ones with much more *contour* than the others in the same series. A high proportion of the ganglion cells in the vertebrate eye appear to respond either to the onset or to the termination of illumination only (Bartley, 1951, pp. 965-9). Whenever the eyes are in motion, whether in the course of large-scale traverses of the visual field or as a result of the physiological nystagmus described by Marshall & Talbot (1942), the patterns with the largest amounts of contour will produce the most excitation of on- and off-fibres. The high stimulating power that results from this phenomenon might account for the high eye-drawing power of patterns like B3 and D3. The influence of some of the other forms of complexity that have been found to affect curiosity and attention in adults (Berlyne, 1957*a*, 1958) must, on the other hand, be attributed to more central processes.

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SKILL AND CHANCE: VARIATIONS IN ESTIMATES OF SKILL WITH AN INCREASING ELEMENT OF CHANCE

BY JOHN COHEN, E. J. DEARNALEY AND C. E. M. HANSEL

Department of Psychology, University of Manchester

Experiments with three groups of subjects, ranging in age from 13 to 20 years, were carried out to determine how estimates of success in performance are influenced by (a) varying the subjective assessment of the degree of skill required to succeed in a task, and (b) varying the degree of subjective chance. The findings were as follows. (i) The result of introducing an element of subjective chance into a task requiring some skill is to make the estimates of success in the task progressively reflect the element of subjective chance as it increases in magnitude. (ii) This effect occurs regardless of the degree of skill subjectively felt to be required for the task. (iii) The values of subjective probability of success in performance given by the older subjects are not significantly different from the products of their corresponding values of $\psi(\text{skill})$ and $\psi(\text{chance})$. In the circumstances of these experiments, therefore, the subjective probabilities based upon the subjects' estimates are equivalent to those which the experimenter could independently arrive at if he only had available to him the values of $\psi(\text{skill})$ and $\psi(\text{chance})$ and had multiplied them. (iv) In the case of younger subjects aged 13–14 years, the subjective probabilities of success in performance are somewhat larger than the products of the two separate subjective probabilities. This may be due to a tendency on the part of younger persons to be more confident of success when their estimates relate to a *single* situation involving two sources of uncertainty as compared with when they relate to two *separate* situations, each involving its own source of uncertainty.

I. INTRODUCTION

Tasks in general vary from one extreme at which the performer believes the outcome to be almost entirely due to factors outside his control, to the other extreme at which he believes the outcome is almost entirely due to his personal skill, the element of chance being, he believes, negligible. These beliefs do not necessarily correspond with the 'objective' elements of chance in the situation or with the performer's true skill. Thus, some gamblers believe they have devised systems to 'beat' the play of the roulette wheel; or a person may try to explain away his scholastic failure by bemoaning his bad luck. We are here concerned with performances calling for some degree of skill and involving an element of chance which favours or handicaps the performer. The performer may not consciously and explicitly make an estimate of the factor of chance. In principle, however, it is possible to arrange situations so that his estimates of success can be analysed to reveal the relative influence of two component estimates of success. One of these estimates would depend on how skilful he thinks he is when he believes chance is negligible. This we call $\psi(\text{skill})$. The other estimate would depend on how lucky he believes he would be when he recognizes that the outcome depends on chance. This estimate is made when the task is so simple that he has no doubt about his ability to succeed in it when chance is absent. This second estimate we call $\psi(\text{chance})$. The aim of the present experiment is to determine how subjective probabilities of success in performance are affected by variation in these two component estimates. (In a previous publication—Cohen, Dearnaley & Hansel, 1956—we denoted subjective probability of success as ψ_s in contrast to ψ_f which represented subjective probability of failure.)

II. PROCEDURE

The task consisted in throwing steel darts to hit a circular target and remain there. The target was vertically placed on a wall. There were five sets of five darts. The sets included different numbers of darts (0, 1, 2, 3 or 4) which were blunted so that they could not possibly remain on the target however hard they were thrown. The skill required by the task was varied by altering the diameter of the target. There were twelve target sizes ranging from the largest with a diameter of 28 in. to the smallest with a diameter of 1 in. The subject's distance from the target in the experiment was adjusted during a preliminary practice period, the distance being such that he would probably always succeed at the largest target and probably never at the smallest. This distance varied from subject to subject.

Stage (i). In order to obtain a measure of ψ (skill) for each subject in relation to each target, the targets were successively presented to him first in ascending order of size and then in descending order. (An anticipation error was found as would be expected from this procedure.) Each time the target was presented the subject estimated how many times out of five attempts he thought he would succeed when all five darts were sharp. Since it was necessary to present targets yielding the same values of ψ (skill) for all subjects, five target sizes were chosen for each subject at which his mean estimates of success were 1, 2, 3, 4 and 5 times out of 5, respectively, or as near as possible to these values. This provided, for each subject, five targets representing five levels of ψ (skill). Although the actual target sizes varied from subject to subject they had, respectively, the same levels of ψ (skill) for all. We thus obtain a measure of ψ (skill).

Stage (ii). Having established five sizes of target corresponding to five arbitrary chosen values of ψ (skill), when all five darts were sharp and the chance element of drawing a blunt dart was therefore ruled out, we now determined each subject's estimates of success at varying levels of chance, i.e. when there was a possibility that he might draw one or more blunt darts. These estimates were obtained with the largest target at which the subject was sure he would always succeed provided he had five sharp darts. Naturally, before choosing a dart, the subjects could not see which one was blunt and which was sharp. They always knew how many blunt darts there were in each set before choosing a dart. They understood too that the dart would be replaced in the set at random after use.

Stage (iii). The final step was to obtain estimates from each subject when the target size, and hence ψ (skill) and the element of chance, in terms of the number of blunt darts, were varied simultaneously. Thus at each of the remaining four levels of subjective difficulty, estimates were obtained when there were 1, 2, 3 or 4 blunt darts in a set of five darts before each throw. The estimates at this stage and at the preceding one were obtained in random order. This procedure was employed with thirty-one school-children aged 13-14 years and with seven undergraduates.

In a second comparable experiment with eleven undergraduates the task consisted in throwing a bead into a beaker. In order to obtain a measure of subjective difficulty, the beakers presented were of various sizes, thus matching the varying size of the targets of the previous experiments. The subject had to draw a bead from a bowl containing five beads each of which could be either red or green. The varying proportions of red and green beads were known to him, though he was unable to see what was in the bowl. The element of chance was introduced by allowing the subject to throw a bead only if he drew a red one. Thus, in this second experiment ψ (skill) is based upon estimates when the subject is sure that he will always draw a red bead, and ψ (chance) is based upon estimates when the subject is sure he can succeed in throwing a bead into the beaker, but realizes that whether or not he draws a red bead is entirely outside his control.

III. RESULTS

All the estimates in what follows are converted from the number of successes out of five to fractions of unity. These fractions of unity we take to be a measure of subjective probability. The five targets arbitrarily chosen, when the subject knew that all the darts were sharp, were such as to obtain values of ψ (skill) as near as possible to 1.0, 0.8, 0.6, 0.4, and 0.2. The experimental average values of ψ (skill) for these five chosen targets were found to be 1.0, 0.80, 0.61, 0.39 and 0.19. These appear in the first column of Table 1. The corresponding average values of ψ (chance), when the subject knew there was a smaller or larger chance of drawing a blunt dart before each throw were found to be: 1.00, 0.73, 0.54, 0.37 and 0.24. These are shown as column headings in Table 1. They correspond to

a priori probabilities 1.0, 0.8, 0.6, 0.4, 0.2 of drawing a sharp dart and were obtained when the subject was aiming at the largest target, that is, when he thought he would always succeed with five sharp darts.

A comparison of these two latter sets of figures, viz. of $\psi(\text{chance})$ with the *a priori* values show that $\psi(\text{chance})$ is greater than the *a priori* value when this is small and is less than the *a priori* value when it is large. The two sets of figures correspond most closely when the *a priori* value is about 0.3. This supports the outcome repeatedly obtained in earlier experiments (Cohen & Hansel, 1956), which suggests that we tend to overestimate our capacities when we think the task is hard and to underestimate them when we think it is easy, an effect that results in a relative contraction of the subjective probability as compared with the *a priori* scale (Cohen & Hansel, 1957*a*).

Our primary concern here is to determine the variation in estimates of success with variation in the two components, $\psi(\text{skill})$ and $\psi(\text{chance})$. The figures in Table 1 are presented with this in view. In the context of the present experiment $\psi(\text{chance})$ refers to the subjective probability when the probability of drawing a sharp dart varies. This was determined for a target at which the subject thought he would always succeed if he had five sharp darts. Similarly $\psi(\text{skill})$ refers to the subjective probability of hitting the target at any given size when the *a priori* probability of drawing a sharp dart is 1.0 (i.e. when all five darts are sharp).

Table 1. *Mean estimates of success in performance (expressed as subjective probabilities) when both $\psi(\text{chance})$ and $\psi(\text{skill})$ vary*
($N=31$ aged 13-14)

$\psi(\text{skill})^*$	$\psi(\text{chance})^\dagger$			
	0.73	0.54	0.37	0.24
0.80	0.62	0.48	0.33	0.21
0.61	0.56	0.43	0.31	0.24
0.39	0.40	0.29	0.24	0.15
0.19	0.20	0.13	0.12	0.07

* When all five darts are sharp.

† Corresponding to *a priori* probabilities 0.8, 0.6, 0.4 and 0.2 of drawing a sharp dart.

It would be reasonable to expect estimates of success, at any given size of target, to decline as the chance of choosing a sharp dart diminishes. In other words, at any given value of $\psi(\text{skill})$, estimates of success would decline with decrease in $\psi(\text{chance})$ and vice versa. This was, in fact, found to be the case.

The question, however, remains as to the magnitude of decline to be expected. On common sense grounds we should expect the decline progressively to reflect the diminishing values of $\psi(\text{chance})$. If subjective probabilities combined in a manner analogous to mathematical probabilities, their magnitude would be approximately the product of corresponding values of $\psi(\text{skill})$ and $\psi(\text{chance})$. We have computed these products and subtracted them from the corresponding observed values in Table 1. The resulting residuals are presented in Table 2. They are consistently positive. The mean discrepancy between the observed and theoretical values is 0.06 which, contrary to the null hypothesis, is significant at the 0.001 level ($t=7.62$, D.F. = 15, $P<0.001$). The discrepancy is significant but small. We may conclude therefore that the ψ -values in Table 1 are not entirely reducible to the product of two separate subjective probabilities.

Table 1a. *Table of residuals when products of ψ (chance) and ψ (skill) are subtracted from the corresponding observed values*

0.04	0.05	0.03	0.02
0.11	0.10	0.08	0.09
0.12	0.08	0.10	0.06
0.06	0.03	0.05	0.02

When we turn, however, to the results of the similar experiment with the undergraduates we do not find any significant difference between the calculated and observed values. The mean of the residuals is zero. The parallel data for these older subjects are presented in Tables 2 and 2a.

Table 2. *Mean estimates of success in performance (expressed as subjective probabilities) when both ψ (chance) and ψ (skill) vary*

ψ (skill)*	ψ (chance)†			
	0.69	0.46	0.38	0.14
0.83	0.46	0.37	0.26	0.17
0.59	0.49	0.31	0.23	0.11
0.40	0.29	0.20	0.09	0.06
0.15	0.20	0.03	0.03	0.00

* When all five darts are sharp.

† Corresponding to *a priori* probabilities 0.8, 0.6, 0.4 and 0.2 of drawing a sharp dart.

Table 2a. *Table of residuals when products of ψ (chance) and ψ (skill) are subtracted from the corresponding observed values*

-0.11	-0.01	-0.06	+0.06
+0.08	+0.04	-0.01	+0.03
+0.01	-0.02	-0.06	0.00
+0.01	-0.04	-0.03	-0.02

The same agreement between calculated and observed values in the case of the older subjects appeared in the second experiment in which drawing and throwing a bead replaced selecting and aiming a dart. The relevant figures are shown in Tables 3 and 3a.

Table 3. *Mean estimates of success in performance (expressed as subjective probabilities) when both ψ (chance) and ψ (skill) vary*

ψ (skill)*	ψ (chance)†			
	0.69	0.53	0.35	0.18
0.81	0.52	0.40	0.30	0.16
0.60	0.50	0.34	0.22	0.08
0.43	0.23	0.22	0.13	0.02
0.21	0.16	0.09	0.00	0.00

* When all five beads are red.

† Corresponding to *a priori* probabilities 0.8, 0.6, 0.4 and 0.2 of drawing a red bead.

Table 3a. *Table of residuals when products of ψ (chance) and ψ (skill) are subtracted from the corresponding observed values*

-0.4	-0.3	+0.2	+0.1
+0.9	+0.2	+0.1	-0.3
-0.7	-0.1	-0.2	-0.6
+0.2	-0.2	-0.7	-0.4

In venturing to interpret these results we may point to a greater internal consistency in the judgements of the older subjects as compared with those of the younger ones. The judgements of the older subjects in Stage (iii) logically follow from those they made at Stages (i) and (ii). In the case of the younger subjects, however, there is some departure from this logical consistency. The departure may be due to a tendency on the part of the younger subjects, when faced with two sources of uncertainty, to be relatively less affected by it when both sources are present in the same situation than when they occur in two separate situations. More specifically, estimates of their skill made by the younger subjects cannot be divorced in their minds from their feelings about the chances of drawing a sharp dart or not, and vice versa. The older subjects display similar tendencies in the estimates they make in the two different experimental situations, with darts and beads, respectively. Indeed, their estimates may be expressed as a compound of two component subjective probabilities, in a manner analogous to the combination of mathematical probabilities.

The fact that similar results were obtained with two different tasks and different methods of introducing an element of 'subjective chance' indicates that the effects are not specific to a particular situation but appear to have a degree of generality. Hence, the outcome of the experiments with the older subjects is that in a complex situation involving two sources of uncertainty, the subjective probability of success in performance is equivalent to the product of the ψ -values relating to each separate source. Any possibility that the subjects (younger or older) might calculate their estimates in terms of the two components was completely ruled out by the randomizing procedure in the experiment. In any case, the younger subjects could certainly not have known that they could calculate their estimates of success by multiplying two component estimates.

These results suggest a number of problems that need to be investigated, especially in the light of our previous experiments (Cohen & Hansel, 1957*b*) in which we studied the mode of combination of values of ψ (chance) deriving from separate sources of uncertainty. In further experiments we have examined estimates made when (a) there are two or more *a priori* sources of uncertainty simultaneously present, and (b) when there are two or more sources of uncertainty about skill simultaneously present. The results of these subsequent experiments are reported elsewhere (Cohen, Dearnaley & Hansel, 1958).

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EFFECT OF QUINAL BARBITONE, DEXTRO-AMPHETAMINE, AND PLACEBO ON APPARENT TIME*

BY SANFORD GOLDSTONE, WILLIAM K. BOARDMAN
AND WILLIAM T. LHAMON

Department of Psychiatry, Baylor University College of Medicine, Houston, Texas

Quinal barbitone, dextro-amphetamine and placebo were compared with respect to changes produced in perceived duration in ninety normal subjects. A method was employed which permitted quantitative derivation of a subject's concept of *one second*, pre-drug, 30 min. post-drug and 60 min. post-drug. The following findings were obtained. (1) A significant decrease in the clock-measured value of the apparent second with the ingestion of dextro-amphetamine. (2) A significant increase in the clock-measured value of the apparent second with the ingestion of quinal barbitone. (3) A trend toward increase in the apparent second with placebo. These findings were discussed in terms of earlier reports and a theoretical framework was offered.

I. INTRODUCTION

Although alterations in the estimation of time are among the most frequently reported perceptual changes associated with drug states, little quantitative evidence is available which either verifies or refutes published and accepted notions based upon qualitative, observational case study. The lack of experimentally defined alteration in the time sense has led to considerable confusion with respect to the meaning of such words as underestimation and overestimation, physical time and personal time. This paper describes a method of studying the effects of common drugs upon apparent time, where prior qualitative, observational evidence was available to suggest that these agents might influence perceived duration. Sedation is frequently accompanied by the subjective experience of external time 'flying'—the standard physical clock dimension is underestimated. Stimulation, on the other hand, is more frequently associated with the subjective experience of external time 'dragging'—clock time is overestimated.

A method was employed which permitted a quantitative derivation of the concept of one clock-second or Second Estimation Point (S.E.P.). Considerable data on normal (Goldstone, Lhamon & Boardman, 1957; Smythe & Goldstone, 1957) subjects was available, and the method was sensitive to the reported time-estimation changes associated with schizophrenia (Lhamon & Goldstone, 1956; Weinstein, 1956) and lysergic acid diethylamide (Boardman, Goldstone & Lhamon, 1957). It was hypothesized that a drug-induced awareness of apparent slowing of clock time (i.e. time 'dragging') would accompany a smaller S.E.P. A drug-induced awareness of an apparent speeding of clock time (i.e. time 'flying') would accompany a longer S.E.P. in the direction of underestimation.

Drug-induced alterations in apparent duration have been reported with marihuana (Bromberg, 1934), lysergic acid and mescaline (Hoch, Cattel & Pennes, 1952), and nitrous oxide (Steinberg, 1955). Only in the nitrous-oxide study was an experimental, quantitative

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procedure with a group of subjects reported. Steinberg (1955) investigated the differences in estimation of 30 sec. by counting aloud at an estimated speed of one count per second between an anaesthetized and a control group. Both groups demonstrated a significant increase in the apparent 30 sec. after drug with no difference between the two groups. She concluded as follows: 'The drug therefore cannot be regarded as having had any effect on estimating seconds. That the mean scores of both groups should have risen at all may possibly be an artifact attributable to the apparatus used to administer the gas. . . .' The anaesthetized group did demonstrate a change in time estimation following the gas, but the author preferred to dismiss her finding, presumably in favour of the assumption that a placebo is an inert agent with little potential for altering behaviour. The present study reduced the cues available in the common counting procedure and employed a stimulant in addition to the sedative and placebo, assuming that placebos are not always inert and may produce psychological effects which when similar to drug effects may tend to mask the effect of the active agent.

II. METHOD AND PROCEDURE

Apparatus. An electronic timer activated an audio oscillator that delivered, through headphones, a tone of 725 cyc./sec. for durations of between 0.1 and 2.0 sec. at 0.1 sec. step intervals. The amplitude of the tones was comfortable and constant.

Subjects. Ninety medical students and interns were divided into three equal groups, each receiving one of the studied drugs. One group received 0.2 g. quinal barbitone (called in America secobarbital), a second group received 15 mg. dextro-amphetamine, and a third group received a lactose placebo. All drugs were administered orally.

Procedure. Subjects were studied under three conditions: (1) pre-drug, (2) 30 min. post-drug, (3) 60 min. post-drug. They were presented with a series of audible durations in accordance with a method of limits technique and were required to respond to each duration with the report 'more' or 'less', indicating a judgement of more or less than 1.0 sec. Twenty alternating runs of ascending and descending durations were provided starting at 1.0 sec., with 0.1 a sec. step interval. If the subject reported *more* to the initial 1.0 sec. stimulus, the first run was of a descending nature and terminated when he reported *less* three consecutive times. If the subject reported *less* to the initial 1.0 sec. stimulus, the first run was of an ascending nature and terminated when he reported *more* three consecutive times. This procedure was followed for each drug under the pre-drug, 30 and 60 min. post-drug conditions.

The percentages of reports *less* for each duration of each drug condition were plotted on normal probability paper on the assumption that these values were normally distributed and the straight line visually fitted (Dixon & Massey, 1951). The S.E.P. or duration at which the subject reported more and less 50% of the time were derived from these plots.

III. RESULTS

All S.E.P.'s were found to be log normally distributed and the appropriate transformations were made. The obtained pre-drug mean log S.E.P.'s for the quinal barbitone, dextro-amphetamine, and placebo groups of $\bar{1}.7691$, $\bar{1}.8380$ and $\bar{1}.8132$, respectively, did not differ significantly from each other. The antilogs of the mean log S.E.P.'s (geometric mean S.E.P.'s) of these three pre-drug groups (0.59, 0.69 and 0.65 sec., respectively) indicate that all groups tended to overestimate the value of the clock second prior to drug administration. These results correspond to previous findings (Goldstone *et al.* 1957). The differences between the pre-drug log S.E.P., and 30 and 60 min. post-drug log S.E.P.'s were obtained. Table 1 summarizes the mean shift in log S.E.P. for each drug from the pre-drug to the 30 and 60 min. post-drug conditions. A rise in log S.E.P. from pre-drug to a post-drug condition is designated by a *plus* sign and a reduction in log S.E.P. appears with a *minus* sign. Mean log S.E.P. change is also interpreted in terms of percentage change for convenience and clarity. Change in log S.E.P. was examined with a *t*-test for paired observations.

Table 1. *Mean shift in log S.E.P. for each drug group from pre-drug to 30 and 60 min. post-drug in log units and in percentage (a plus sign reflects a rise in S.E.P. and a minus sign reflects a reduction in S.E.P.)*

	Quinal barbitone		Dextro-amphetamine		Placebo	
	Log shift	% shift	Log shift	% shift	Log shift	% shift
30 min. post-drug	+0.0475	+11.9	-0.0430	-10.3	+0.0294	+7.1
60 min. post-drug	+0.0876	+22.2	-0.0597	-15.1	+0.0363	+8.7

The quinal barbitone group demonstrated a significant rise in log S.E.P. after 60 min. ($t=2.83$; $P<0.01$) with a smaller rise noted after 30 min. ($t=1.81$; $P<0.10$). This alteration in apparent duration is in the direction of underestimation and is compatible with a subjective report that time seems to be 'flying'. The dextro-amphetamine group demonstrated a significant reduction in log S.E.P. after both 30 min. ($t=2.04$; $P<0.05$) and 60 min. ($t=2.60$; $P<0.025$). This alteration in experienced duration is in the direction of overestimation and is compatible with a subjective report that time seems to be 'dragging'. The placebo group demonstrated a small but not significant rise in log S.E.P. after both 30 and 60 min. The similar direction of both placebo and sedative groups conforms to the previously reported finding of Steinberg (1955).

The quinal barbitone group differed significantly from the dextro-amphetamine group with respect to change in log S.E.P. after both 30 min. ($t=2.70$; $P<0.01$) and 60 min. ($t=3.84$; $P<0.005$). The placebo group differed significantly from the dextro-amphetamine group with respect to change in log S.E.P. after both 30 min. ($t=2.92$; $P<0.01$) and 60 min. ($t=2.55$; $P<0.025$). No significant differences were obtained between placebo and quinal barbitone after 30 or 60 min., quinal barbitone after 30 min. and quinal barbitone after 60 min., dextro-amphetamine after 30 min. and dextro-amphetamine after 60 min., or placebo after 30 min. and placebo after 60 min. with respect to log S.E.P. change. It is of interest that the increase or decrease in log S.E.P. for each drug was greater after 60 min. than after 30 min.

These results demonstrate a significant decrease in the clock-measured value of the apparent second with dextro-amphetamine; a significant increase in the clock-measured value of the apparent second with quinal barbitone, and a trend toward increase in the apparent second with placebo. The effect of placebo and quinal barbitone upon the apparent second differ significantly from the effect of dextro-amphetamine.

Subjects were asked to report the effects of the drugs and to guess the nature of the medication. Most of the quinal barbitone group reported sedation at 30 min. and all but one experienced the usual symptoms at 60 min. Several subjects had to be awakened to be tested and two fell asleep during testing. None of the dextro-amphetamine group reported stimulation of mental or physiologic processes while five reported drowsiness at 30 min. One subject guessed that he had a stimulant, while three continued to feel drowsy at 60 min. Most of the dextro-amphetamine subjects reported no noticeable drug effects. This suggests that time-sense measurements provide a sensitive method for demonstrating changed mental functioning, even prior to subjective awareness. Ten of the placebo group thought they had a mild sedative and one subject fell asleep.

Subjects were also asked to report their feeling about how consistent they were from test to test. Most reported experiencing the same duration as a second, even though the

clock indicated a marked change. The experienced second had apparently remained the same, providing evidence for the apparent speeded or slowed perception of time. The drug-induced change in the clock-measured second prevailed even when subjects reported continued use of such mental devices as counting or visualizing clock movement.

IV. DISCUSSION

These results quantitatively demonstrate the common qualitative observation of a drug-induced alteration in the estimation of time. The experience of time flying might be interpreted as a relative speeding of the sequence of external events when compared to a personal standard, or frame of reference; while the experience of time dragging might be interpreted as a relative slowing of the sequence of external events when compared to one's standard. Pieron (1952) suspected that appreciation of time was associated with a subjective standard frame of reference or 'time appropriate to our organism'. He thought that changes of internal temperature must modify this standard. François (1928) found that the apparent second was shortened when body temperature rose. Hoagland (1933) verified this finding and hypothesized that increased body temperature should be associated with a speeded internal chemical clock that should correlate with the perception of external time proceeding more slowly, since more 'chemical clock time' would pass in a given physical clock unit than under normal conditions. Pieron (1952) reports that when it is warmer trained bees fetch sugar ahead of time, and when it is colder they are late.

Without committing ourselves to the source and nature of the internal pacemaker, we might make use of Hoagland's and Pieron's formulations about an internal temporal standard. Let us postulate the organism as a clock and organismic time as a function of an event-counting process which is independent of arbitrary physical clock units. These 'events' are hypothetical constructs which at present are not identified in terms of any specific biologic, chemical or psychologic system, but represent discrete recurrences, the counting of which provide us units of personal time. It is further postulated that each organism has a standard rate of countable events which varies, within limits, with modifications of organismic or environmental conditions. It is this variation in event rate which we associate with alteration in perceived time. An earlier study (Smythe & Goldstone, 1957) suggested that at age 8 the learning of culturally determined time standards takes place. Since individual differences probably exist in event rate and personal time, cultural standards permit people to conform their private rates with consensually accepted physical time and allow communication and interpersonal transactions with respect to duration, sequence and order. We suspect that at age 8 a standard and stable event rate becomes identified with newly learned physical time concepts. The speeding or slowing of event rate would modify the correspondence with physical units and alter the physical clock value of the apparent unit. If a rise in body temperature is associated with an acceleration of countable events, more events will transpire in a given unit of clock time. If the given unit (let us say 1 sec.) had formerly been identified with a specific number of events (let us say ten), this number will pass in less time (let us say 0.5 sec.) providing an overestimation of the clock second (in this case, an apparent second or S.E.P. of 0.5 sec.). This formulation corresponds to the obtained findings with raised temperature.

The stimulating effect of dextro-amphetamine also leads to a prediction of a speeded event rate which should be associated with a shorter clock-measured apparent second and perception of 'things slowed down'. On the other hand, the sedative effects of quinal barbitone might lead to a prediction of a slowed event rate which should be associated with a larger clock-measured apparent second and the perception of 'things speeded up'. These predictions were verified by the results of this study. Other reports (Rosenthal & Frank, 1956) have indicated placebo effects that are similar to those of a sedative. It is possible that the small placebo effect and the larger barbiturate effect in the same direction might merely be a function of time passage or the repetition of the test. However, the magnitude and consistency of the barbiturate effect suggests that the drug rather than test conditions was the source of judgement alteration. Subjects studied throughout a single day and regularly for several months (Boardman *et al.* 1957) without a drug did not demonstrate the significant rise in S.E.P. noted with the quinal barbitone group.

The findings of this study conform to those obtained by Steinberg (1955), suggesting that the increased apparent second noted in both the nitrous-oxide and placebo-gas groups was not spurious. It is suggested that a placebo acts much like a sedative in altering perceived durations. These results and the proposed theoretical speculations are in line with Schilder's (1942) description of Sterzinger's findings on retarded and speeded metabolic processes with quinine and thyroxin. Thyroxin administration was associated with relative overestimation of time, while quinine ingestion was associated with a relative underestimation of time.

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EXTREME RESPONSE SETS AS A MEASURE OF INTOLERANCE OF AMBIGUITY

By M. I. SOUEIF

Institute of Psychiatry, Maudsley Hospital, London

A *Personal Friends Questionnaire* was administered to various groups of Egyptian subjects totalling 1028 in all. This questionnaire is composed of seventy items, each of which is to be checked either +2, +1, 0, -1 or -2. Extreme response scores were obtained by counting the number of times +2 or -2 were assigned by each subject. Corrected split-half reliabilities of the extreme response scores for males and females were 0.92 in both cases. The extreme response set scores are suggested as possible measures of intolerance of ambiguity. A general hypothesis was formulated according to which a social group with a higher tension level would earn a higher extreme response score than a social group with a lower tension level. The theoretical formulation of the problem in this way allowed for four predictions, about the effect on extreme response set scores of age, sex, membership of a religious minority and socio-economic status. For verification of the predictions, the social groups were matched for age, sex and religion. The four expectations were, on the whole, fulfilled.

I. INTRODUCTION

Cronbach (1950) has observed that response sets become more influential as test items become difficult or ambiguous. The crucial point, according to him, is to determine whether these response sets 'are characteristics of the individual stable from time to time, or are transient sets which can only be regarded as errors in testing rather than personality characteristics'. Berg (1953) has demonstrated that 'the tendency to choose extreme positions when marking choices on the *perceptual reaction test* (P.R.T.) and the *word reaction test* (W.R.T.) is acceptably stable over test-retest periods up to 15 days'. Rundquist (1950) reported a correlation of 0.40 ($N=111$) between this tendency 'in two situations, one immediately following the other'. As Berg (1953) puts it, 'the indications are that at least some forms of response sets are reflexions of personality traits'. Some evidence that this is the case appears in scattered studies. Lewis & Taylor (1955) have demonstrated the tendency for high anxious subjects (who scored twenty-five or above on the Taylor Anxiety Scale) to have higher extreme responses (*like very much, dislike very much*) on the P.R.T. than the low anxious subjects (scoring seven or below). Berg & Collier (1953) have reported significant differences between the mean extreme responses of high anxious males, high anxious females and generally neurotic males, and low anxious males, low anxious females and normal males, respectively. They demonstrated also that White females and Negro males had significantly higher extreme responses than White males. It might be interesting to note that Terman & Miles (1936, p. 38) called attention 'to a persistent sex difference in the frequency with which male and female subjects check extreme responses on tests of emotional and ethical attitudes'.

The nature of the extreme response sets seems to point to the feasibility of considering the extreme response set scores as a measure of *intolerance of ambiguity*. In reporting findings from her extensive researches, Frenkel-Brunswik (1951, p. 394) has summarized her general results as follows. 'We found evidence, in certain groups of our subjects, of intolerance of ambiguity in the perceptual as well as in the emotional and social areas.' In these groups of subjects 'feelings are expressed in terms of the ends of a continuum

rather than of a continuum proper'. Frenkel-Brunswik further emphasized that 'underlying anxiety issuing from the confusion of one's social identity and from other conflicts is apparently so great that it hampers individuals (in such groups) in facing' external ambiguity (p. 395).

The main objective of this investigation was to study the influence of being a member of a certain social group on intolerance of ambiguity, as measured by extreme response set scores. A general hypothesis was formulated as follows. *Social groups, with different levels of tension, differ from each other with regard to their average intolerance of ambiguity, as measured by number of extreme scores. Other conditions being equal, a social group assumed to have a higher tension level (due mainly to feelings of insecurity) would tend to make more extreme responses when compared with another social group assumed to have a lower tension level.* The concept of *tension* has been widely used in modern psychological literature (Eysenck, 1953, pp. 299-303). In this investigation, by ascribing a level of tension to social groups, a certain average tension level was assumed for the members of each of those groups. With this general hypothesis in mind the following predictions were formed. (a) Adolescents are expected to produce more extreme responses than adults. (b) Christians (as members of a religious minority group in Egyptian society) are expected to have more extreme responses than Moslems. (c) Females are expected to have more extreme responses than males. (d) Members of the lower middle class are expected to produce more extreme responses than members of the upper middle class.

II. PROCEDURE

A questionnaire on the characteristics of personal friends, hereafter referred to as P.F.Q., was constructed (see Winslow & Frankel, 1941). It was composed of seventy items representing all the views expressed by twenty Egyptian subjects (fifteen males and five females, ranging in age from 16 to 28) in essays on friendship. Most of the items were single words, e.g. co-operative, dependable, gloomy, ignorant. The items followed each other randomly, without any predetermined arrangement. The respondent was to consider whether in making friends with a given person he (or she) considered each characteristic in the list as *very necessary*, *desirable*, *indifferent*, *undesirable*, or *definitely opposed*. Numerical values assigned to these various responses ranged from +2 through +1, 0, -1 to -2. The P.F.Q. was presumed to be fairly 'unstructured or ambiguous in the sense that there were no "right" or "wrong" answers' (Berg, 1953). Finally, each test was scored for extreme responses by counting the number of times an extreme response (either +2 or -2) was assigned.

To determine the reliability of extreme response sets, a split-half method (i.e. splitting the whole seventy items into odds and evens) was adopted. Estimated reliability for males was 0.85 ($N=100$). Corrected for length by the Spearman-Brown formula it turned out to be 0.92. The corresponding values for females were 0.86 and 0.92, respectively ($N=100$).

An attempt was made to verify the assumption that extreme response scores were not affected by the fact that some of the subjects signed their responses. The P.F.Q. was administered to 338 adolescent females (Moslems). The accompanying instructions were that they were free to sign their responses or to leave them unsigned. Responses were signed by 174 subjects, while 164 left them unsigned. The medians of both groups were compared, and the difference was not significant. (The distribution of scores in the non-signing group was significantly positively skewed, $t=2.24$.)

The P.F.Q. was administered (in its Arabic version) to various groups of Egyptian subjects totalling 1028 in all. They ranged in age from 12 to 46 with a median of approximately 17 years. Most of the subjects were members of the urban middle class with its lower and upper strata. They were drawn from students of Cairo University, from Egyptian secondary school classes in Cairo, Alexandria and some other big towns nearer to the rural districts, and from some Egyptian clubs, during the years 1951-2. The P.F.Q. was usually administered to groups of forty to fifty subjects at a time. The questionnaire sheets had the necessary instructions printed at the top. The subjects were always told that they were free to sign the questionnaire or to leave it unsigned.

III. RESULTS AND DISCUSSION

To control the effect of certain variables which might have interfered and distorted the results, it appeared desirable to match the groups to be compared, for age, sex and religion. Inspection of Tables 1 and 2 indicates that on the whole the predictions were verified. Thus, for the first prediction, the median score of extreme responses of the

Table 1. *Extreme response scores: means and medians*[†]

Group	N	Mean	S.D.	Median
Adolescents, females, Moslems	338	30.58	9.94	30.12
Adolescents, females, Christians	52	36.42	11.99	34.50
Adolescents, males, Moslems	366	33.38	10.91	33.05
Adolescents, males, Christians	31	32.32	10.94	—
Early adolescents, females, Moslems	54	31.18	9.28	—
Early adolescents, females, Christians	10	42.30	13.64	—
Early adolescents, males, Moslems	235	33.83	10.61	—
Early adolescents, males, Christians	17	34.88	10.39	—
Late adolescents, females, Moslems	284	30.50	9.91	—
Late adolescents, females, Christians	42	35.07	11.11	—
Late adolescents, males, Moslems	131	32.39	11.43	—
Late adolescents, males, Christians	14	29.14	9.67	—
Adults, females, Moslems	67	30.88	9.50	29.30
Adults, females, Christians	25	36.00	10.77	—
Adults, males, Moslems	114	27.31	8.98	25.57
Adults, males, Christians	35	27.28	8.23	—
Upper middle class (males, adolescents, Moslems)	23	31.30	10.41	—
Lower middle class (males, adolescents, Moslems)	21	48.10	10.80	—
Signing group (adolescent females, Moslems)	174	30.70	9.56	30.37
Non-signing group (adolescent females, Moslems)	164	30.45	10.32	29.69

[†] The medians are stated where they are needed for statistical comparisons owing to skewness of distributions.

Table 2. *Level of significance of differences in incidence of extreme responses:† means and medians*

Groups compared	Statistics compared	t
Adolescents, males, Moslems—adults, males, Moslems	Medians	5.91***
Adolescents, males, Christians—adults, males, Christians	Means	2.06**
Adolescents, females, Moslems—adults, females, Moslems	Medians	0.51
Adolescents, females, Christians—adults, females, Christians	Means	0.15
Christians, males, adults—Moslems, males, adults	Medians	0.92
Christians, males, adolescents—Moslems, males, adolescents	Means	0.42
Christians, females, adults—Moslems, females, adults	Means	2.09**
Christians, females, adolescents—Moslems, females, adolescents	Medians	2.00**
Females, adults, Moslems—males, adults, Moslems	Medians	2.09**
Females, adolescents, Moslems—males, adolescents, Moslems	Medians	3.06**
Females, adults, Christians—males, adults, Christians	Means	3.33***
Females, adolescents, Christians—males, adolescents, Christians	Means	1.58
Females, early adolescents, Moslems—males, early adolescents, Moslems	Means	1.84
Females, late adolescents, Moslems—males, late adolescents, Moslems	Means	1.63
Females, early adolescents, Christians—males, early adolescents, Christians	Means	1.43
Females, late adolescents, Christians—males, late adolescents, Christians	Means	1.91*
Lower middle class, adolescent males, Moslems—upper middle class, adolescent males, Moslems	Means	5.12***
Signing group, females, adolescent Moslems—non-signing group, females, adolescent Moslems	Medians	0.50

[†] One tail test in the cases where differences agreed with predictions.

* Significant at the 5% level.

** Significant at the 2% level.

*** Significant beyond 0.001 level.

adolescents turned out to be significantly higher than the median score of adults in the case of Moslem males (beyond 0.001 level). (The medians were used for comparison whenever the distribution of scores in at least one of the two compared groups was significantly skewed.) In the case of Christian males the mean score of adolescents was also significantly higher than the mean score of adults (beyond 0.02 level, using one tail test). In the case of Moslem females the difference between the medians was not statistically significant though it was in the predicted direction. That also was the case with the Christian females. The second prediction was supported in the case of adult and adolescent females. Thus, the mean score of Christian female adults and the median of Christian female adolescents were both significantly higher than the corresponding mean and median of Moslem female adults and adolescents, respectively (both significant beyond 0.02 level). Comparing the scores of Christian adult males and Moslem adult males it was found that the median of the former was higher than that of the latter. This difference, however, did not reach an acceptable level of significance. In the case of adolescent males the difference between Christian and Moslem means was contrary to prediction, though not significant.

The third prediction was supported in the case of adult Moslems and Christians. In those two groups the median of the females was significantly higher than the median of the males. In the case of the Moslem group the difference was significant beyond the 0.02 level, and in the case of the Christian group it was significant beyond the 0.001 level.

The difference between the medians in the Moslem adolescents group turned out to be statistically significant in a direction opposite to that predicted. Thus, the median of the males was significantly higher than the median of the females. This unexpected result warranted a closer look at the distributions of the groups compared. With regard to chronological age the two groups could not be considered homogeneous. Whereas 64% of the males fell into the early adolescence period (under 16), only 16% of the females fell into this category. It was felt desirable to break down each of the two groups into its early and late component subgroups. Comparing the early and the late female subgroups to the corresponding early and late male subgroups did not change the direction of the results, but rendered them not statistically significant.

In the case of Christian adolescents the mean score of the females was higher than the mean score of the males, although the difference was not significant. When comparing the mean score of the late adolescent females with the mean score of the late males, the former was significantly higher (at 0.05 level).

The specific method adopted to verify the fourth prediction was necessitated by certain facts of the social situation. From among the sample of Moslem male adolescents two groups of twenty-three and twenty-one subjects were selected for comparison. The two groups belonged to two secondary schools which differed from each other with regard to the socio-economic background of their populations. One of the two schools was situated in a district inhabited by well-to-do families, and received the majority of its students from among upper middle-class families. The other was situated in a native district, mostly inhabited by lower middle-class families, and received its students largely from among lower middle-class families. Moreover, inspection of the fathers' occupations in the two groups seemed to point to the feasibility of considering them as belonging to those two different socio-economic classes. The two groups were matched for age. The mean age for the upper group was 14.3 (± 0.86) and for the lower group 13.9 (± 1.02). Comparing

both for extreme responses the lower group earned a strikingly higher mean score than the upper group (significant beyond 0.001 level).

In general, the results of this investigation confirm the hypothesis that a social group with a higher tension level tends to manifest higher intolerance of ambiguity, in terms of extreme responses, than a social group with a lower level of tension. The results are, on the whole, in line with and further illuminate the results established by Berg & Collier (1953). Experimental control of age and socio-economic status could possibly have helped in establishing more consistent findings in their case. The results of this investigation, however, seemed to deviate from the theoretical expectation in one respect: Instead of finding that female Moslem adolescents made significantly higher extreme responses than the male Moslem adolescents, the opposite was found to be the case. This unexpected result might be due to the interference of the socio-economic variable which could not be controlled. A general inspection of the fathers' occupations in both samples tended to verify this hypothesis, which, however, needs further investigation.

The theory elaborated in this investigation is consistent with several theoretical suggestions introduced by Frenkel-Brunswik (1949, 1951) through her clinical and experimental researches. In reporting findings about children who were markedly intolerant for perceptual ambiguity, she emphasized that: 'Data on the parents of the children in the rigid, intolerant group reveal that it is their feeling of social and economic marginality in relation to the group to which they aspire from which ensues the desperate clinging to external and rigid rules. . . ' (1949). She further suggests that: 'It may well turn out upon further evidence that intolerance of perceptual ambiguity is related to a broader psychological disturbance of which prejudice. . . is but another manifestation.' The concept of 'higher tension level', as utilized in this investigation, is assumed to provide that 'broader psychological' basis. From this standpoint, the findings by Lewis & Taylor (1955) and Berg & Collier (1953) that maladjusted subjects tend to have more extreme responses than normal subjects are congruent with the present findings, and can be accounted for on the same theoretical basis.

Whether positive association can be expected between 'intolerance of ambiguity' as defined by extreme response set scores on the P.F.Q. and that same tendency as defined by perceptual measures such as the 'changing figures test' or the 'design preference test' (French, 1955) is a matter for further investigation. Concerning the present study the main objective has been achieved. Extreme response scores, utilized as a measure of intolerance of ambiguity, were found to reveal substantial differences between social groups with different tension levels.

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DRIVE STRENGTH AND ADAPTATION TO STRESS

By H. M. B. HURWITZ AND J. ROWELL

Psychology Department, Birkbeck College, London

Support for the hypothesis that adaptive responses reduce the drive component in behaviour was found in an experiment involving two groups of rats placed, for different periods, in a water tank. After being detained in a delay chamber, the rats were allowed to escape after making a brightness discrimination. A discrimination reversal test was undertaken in order to establish whether the original learning had been affected by the length of the water-detention period. It was found that a long prediscrimination detention period facilitated reversal learning. This result was taken to support the major contention of the paper, that prolonged exposure to a potential stress situation encouraged the elaboration of drive-minimizing responses and, in consequence, favoured discrimination learning.

I. INTRODUCTION

When an animal such as the laboratory rat has been deprived of food, drinking water, a sexual mate, warmth, light, etc., the intensity of its goal-directed behaviour is often said to be related to the extent of the deprivation. Hull (1943) has generalized these observations by claiming that 'drive strength' (D) is a function of the preceding degree of deprivation. As a first attempt at a generalization, this statement has much to recommend it. In the case of food-deprivation at least, the experimental evidence supports the principle (Yamaguchi, 1951; Keller & Schoenfeld, 1950). However, some experimenters (e.g. Kellogg, 1941) have maintained that the repetition of a typical drive-inducing situation leads to modifications in the hypothesized drive-state. In these experiments it appears that continued and repeated exposure to electric shock reduces the motivational efficacy of this stimulus. Consequently, the animal's performance in successive trials of a learning experiment may be conceptualized as the outcome of two factors: increase in habit strength (Hull, 1943) and reduction in 'activation level' (Woodworth & Schlosberg, 1954). However, it appears extraordinarily difficult to obtain clear evidence for the latter part of this hypothesis, i.e. for the suggestion that responses emerge which function to reduce drive level. The present experiment represents a first step towards a solution; we were able to obtain direct evidence that the effect of stress was to encourage the development of a set of responses which ameliorated the stress conditions and minimized typical stress reactions.

Our primary objective of isolating the development of stress adaptive behaviour was undertaken in the following manner: experimentally naïve rats were placed into a water tank for *different* periods and, thereafter, required to perform a brightness discrimination. On making the correct choice, the animals were allowed to escape from the tank by landing on a raised, dry platform. Depending on one's theoretical orientation, two conflicting predictions arise. (a) On the basis of the simple equation that duration of stress is correlated with drive strength, a long detention period should intensify the drive to escape from water, raise the reward effect of escape, and increase the 'magnitude of reinforcement' factor for the instrumental responses leading to escape. These factors should combine to result in the formation of a *stronger* discrimination habit than following a shorter water-detention period; a discrimination reversal test should show that the

discrimination requires *more* trials to be reversed after high-drive training than after low-drive training. Further, since in Hull's 1943 theory the magnitude of reinforcement indirectly determines the rate of habit-strength growth, the high-drive condition should lead to fewer acquisition trials. (b) The point of view defended in the present paper leads to a different prediction. Increasing the detention period favours the elaboration of adaptive responses which, over a succession of trials, minimize the stress state, i.e. modify drive strength. The result is twofold. First, in terms of the number of acquisition trial required to bring the brightness discrimination habit to its appropriate performance level, there need be no difference between the two groups of subjects. As regards the ease of discrimination reversal we can, as yet, make no definite predictions. Secondly, our position commits us to the prediction that there are *qualitative* differences in behaviour under short and long detention periods, i.e. that the adaptive responses to being placed in water will differ markedly.

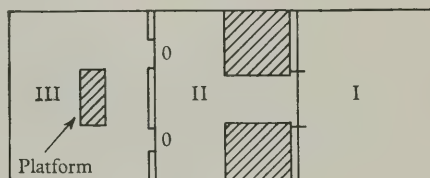


Fig. 1. Plan of the apparatus: I—delay chamber; II—choice chamber; III—escape chamber. Each chamber was divided by a movable partition. Above the partitions separating the choice and escape chamber, two lights were fixed which were switched on in random order during the brightness discrimination trials.

II. THE EXPERIMENT

Subjects. Ten hooded male rats, 3–5 months old at the beginning of the experiment, and drawn from the inbred colony maintained by the Department of Psychology, were used in the present study. The animals were allotted randomly to each of the two experimental groups.

Apparatus. A 3 ft. long water tank, 14 in. deep and $14\frac{1}{2}$ in. wide, was divided into three sections by two partitions (see Fig. 1). The first partition was positioned 15 in. from one end of the tank. It contained a single shutter, 8×6 in., placed centrally, at water level. The animal could swim from the first chamber (delay chamber) into the second chamber (choice point) when the shutter was opened by the experimenter. The second partition was placed 12 in. from the other end of the tank, and contained two shutters, each 4×6 in., at the right and left extremes. An enclosed lamp-holder was fastened above each shutter. In this way, the doors could be illuminated during brightness discrimination trials. When the shutters were opened, the animal could swim from the choice point into the escape compartment. The escape chamber was 9 in. long and contained a floating platform onto which the animal scrambled to escape from the water. The water was $6\frac{1}{2}$ in. deep and was kept at 65–69° F.

Procedure. Both experimental groups received the same preliminary training. (1) Each animal was put, in turn, into the water in the escape chamber. As soon as it had scrambled onto the escape platform it was removed from the apparatus and returned to its home cage. This routine was repeated three times. (2) On the next three trials, the animal was placed in the *choice point* chamber, both shutters being open and a free choice of escape routes thus being available. (3) For the next three trials all the shutters were kept open; the animal was placed in the *delay chamber* and could effect its escape by swimming through all three chambers.

After preliminary training, the experimental conditions were as follows. Each group had to swim through the three chambers and select the brighter of the two shutters in order to effect an *immediate* escape. A wrong choice merely delayed escape by a few seconds, since the animal could swim to the other side of the partition and clamber through the correct, open shutter. Group A was detained in the delay chamber for 10 sec. during training trials; group B was detained for 40 sec. Twenty-five training trials were given: approximately five trials spaced by 10 sec. per session.

At the conclusion of the training period, a further twenty-five trials were run. On these occasions the correct shutter was the darker of the two (i.e. habit reversal) and each group was detained for 25 sec. in the delay chamber. Records were obtained of (a) the total number of errors made by each animal during training and reversal trials, (b) the time taken from the opening of the first shutter to the animal's arrival in the escape chamber, and (c) the manner of dealing with the detention period.

III. RESULTS

Error and time scores during training trials

In terms of the total number of errors made by both groups during training trials, no difference in the rate of discrimination learning could be established. A test for difference in learning rate as measured by reaction times also failed to indicate a statistically significant difference between group A and group B.

Reversal training

A U-test (Wilcoxon, 1945) analysis of the error scores during reversal training indicated a difference between group A and group B which was statistically significant at the 0.02 level. Group A (short-detention period during training) required more trials to reverse the discrimination than group B. The median score for group A was thirteen errors and for group B, seventeen errors.

Behaviour during detention—adaptation responses

During *preliminary* training trials, all animals exhibited typically stressful behaviour, i.e. great activity, urination, rapid heart beat, whining, defecation, etc. Mowrer & Viek (1948) suggest that this kind of behaviour is highly analogous to 'fear from a sense of helplessness'. On being placed in the water, the animals jumped up walls, splashed and thrashed about frantically and attempted to hold fast to any ledge. In the *training* trials proper, these severe stress signs were greatly reduced and, at the end of the training period, had almost completely disappeared. Further, towards the middle of the training period, it became clearly noticeable that group A was very much more *active* in the delay chamber. This activity consisted of vigorous swimming and clawing at the exit door. Group B, on the other hand, developed a tendency to remain perfectly motionless and apparently relaxed for as long as 30 sec. During the final few seconds, group B became active, swimming gently towards the opening door. All five animals in this group showed this behaviour.

During *reversal* training, when both groups were detained in the delay chamber for 25 sec. before being admitted to the choice chamber, *group A developed the adaptive behaviour previously observed in group B, whereas group B reverted to the pattern exhibited by group A throughout the final portion of the training period.*

IV. DISCUSSION

Turning first to the prediction derived from Hullian theory, we see that our results are at variance in a number of ways. Thus, the longer detention period did *not* influence the rate of learning the original discrimination, i.e. a long detention in water does not necessarily raise drive-level, or increase the magnitude of reinforcement for escape responses. And again, the reversal test failed to show that longer detention in the original

learning trials resulted in a stronger habit, i.e. that it would require more trials to acquire the reverse discrimination following long detention than following short detention in the water. But the case made out in the 'Introduction' for assuming that when animals are subjected to a stressful situation they tend to develop adaptive responses which serve to minimize the drive component in behaviour, seems well supported by the present evidence. In the present experiment we have been able to pin-point the adaptive responses which displace or interfere with the original stress reactions and have, thereby, added substance to Kellogg's (1941) suggestion that the motivational efficacy of stress-arousing stimuli becomes attenuated. There are two things worth noting: first, the adaptive responses develop rather slowly, i.e. they reached their effectiveness only after a number of trials. Secondly, the adaptive responses are, in their turn, modifiable and adapted to the nature of the task. Thus, as already indicated, when the detention period for group B was changed from 40 to 25 sec., the adaptive responses changed, i.e. instead of remaining motionless for about 30 sec., the animals engaged in calm unruffled paddling. Our reason for calling these responses 'drive reducing' or 'drive modifying' derives from the more generally accepted description of the role of drives in regulating behaviour. It is said that behaviour varies both in intensity and directness, and that drive relates to the former quality, whereas 'motive' includes both qualities (see Marx, 1956; Hebb, 1955). The function of the responses here singled out for examination clearly affect the *intensity* of the performance, and in fact reduce it. However, we have also shown that, in addition to modifying the intensity of behaviour, the adaptive responses influence the quality of learning. Thus, group B's superior performance on the reversal test seems intimately related to their wider adaptive response repertoire. This inference is supported by the observation that during the initial reversal trials, group A elaborated *new* adaptive patterns prior to exhibiting any signs of learning the reversed discrimination.

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THE MEANING OF 'INTELLIGENCE' TO LEBANESE TEACHERS

BY JACK D. KEEHN AND E. TERRY PROTHRO

Psychology Department, American University of Beirut

The question of the meaning of *intelligent* for English-speaking Lebanese teachers was explored by having twelve teachers rate students in nine classes on eleven scales, including a scale of intelligence. A factor analysis was made of these ratings. It was found that judgements of intelligence covaried with judgements of conscientiousness, thoughtfulness, persistence, and emotional stability, and that such judgements were relatively independent of judgements of cyclothymia and friendliness. The results of this analysis were similar to the results of comparable American studies. This investigation would seem to point up the value of the factor analytic approach to the problem of meaning.

I. INTRODUCTION

In an investigation of the predictive efficiency of certain 'culture-free' intelligence tests including Cattell's *Culture-Free Test* (1944), Raven's *Progressive Matrices* (1938) and the French D48 (1952) version of the *Dominoes Test* (cf. Vernon & Perry, 1949), Keehn & Prothro (1955) used as one of their criteria the judgements of bilingual Lebanese teachers. There was little relationship between scores on the tests and intelligence ratings by the teachers. This lack of relationship could be interpreted as reflecting adversely on the efficacy of the tests, but it might also raise some question of the manner in which the teachers use the term *intelligent*. Teachers' ratings with respect to intelligence cannot be used as criteria in cross-cultural investigations unless teachers in different cultures (or sub-cultures) are using the term similarly. If, for example, teachers in one culture judged an energetic, loquacious person to be intelligent while those of another culture thought of such a person as rather unintelligent, then ratings could hardly serve as criteria in both cultures.

Empirical data on the manner in which *intelligent* is used can be gathered by having judges rate a number of individuals on several different traits. Factor analysis of the ratings will then show the patterns of interrelations between trait names. If it were found that judgements of *cheerful* and *dominant* covaried regularly with those of *intelligent* for different raters and ratees in one culture, then some inference could be made about the 'meaning' of the term *intelligent* in that culture.

Osgood (1952, 1954, 1955) has made extensive use of the factor analytic approach to the problem of meaning. He maintains (1952, p. 227) that 'judgment can be conceived as the allocation of a concept to an experimental continuum' and that 'many different experiential continua...are essentially equivalent'. Thus judging a pupil's intelligence involves treating him as the 'concept' and treating *intelligence* as an 'experiential continuum'. Moreover, other continua, such as *thoughtfulness*, might be shown by analysis to be essentially equivalent to the *intelligence* continuum. In this fashion, then, the meaning of *intelligence* can be determined by locating the term in a 'semantic space' which is described in terms of the dimensions emerging from factor analyses.

Data are already available on the factor analysis of rating scales with American raters and ratees. Cattell's work on the description of personality, although directed toward the

discovery of source traits, revealed a great deal about the use of *intelligent* by his raters when he demonstrated that one of his source traits included *intelligent, conscientious, thoughtful, and emotionally stable* (1946, p. 313). That is, American raters judging many different individuals showed a definite tendency to rate subjects as *conscientious* and *thoughtful* whenever they rated them *intelligent*. The 'meaning' of *intelligent*, then, has much in common with the 'meaning' of these other terms, at least for these American judges.

In order to determine something about the 'meaning' of *intelligent* to English-speaking Lebanese teachers, twelve such teachers were asked to rate their pupils on a series of scales. The scales were then factor analysed to see what general tendencies there were to respond similarly to different trait names.

II. METHOD

The study was carried out at the International College located in Ras Beirut, Lebanon. The pupils of this institution are Arabs, but English is the language of instruction for most of the courses. The instructional staff is largely made up of bilingual Arabs, and the administration is American. The word 'College' in the title of the institution is used in the European sense. That is, work is offered up to the American university sophomore level.

Teachers in this college were asked to rate pupils on the following traits: friendliness, intelligence, emotional stability, dominance, cyclothymia, sympathy for others, conscientiousness, co-operativeness, persistence, conventionality, thoughtfulness, and trustfulness. The list was based in part on Cattell's factorial studies of personality ratings (1946, pp. 313 ff.). For each trait or characteristic, a five-point scale was constructed with statements indicating the five steps. The scale for intelligence, for example, read as follows: 'highly intelligent'; 'above average intelligence'; 'average intelligence'; 'rather below average intelligence'; and 'unintelligent'. The scale for cyclothymia placed 'very cheerful and talkative' at one extreme and 'very quiet and seclusive' at the other extreme. The word *cyclothymia* did not appear on the scale.

Ratings were made by twelve teachers on nine classes of students. These twelve teachers were among those participating in the previous study of intelligence tests (Keehn & Prothro, 1955). Teachers rated only those classes in which they had ample opportunity to get acquainted with each pupil. There were from twenty-eight to thirty-one pupils in each class.

III. RESULTS

Most of the teachers omitted the item on trustfulness, so this item was excluded from the analysis. The other eleven ratings on each class were intercorrelated separately for each teacher. The fourteen resulting correlation matrices were factor analysed separately. In each case virtually no significant residuals remained after the extraction of two centroid factors. These factors were rotated independently and blindly for each analysis. Rotations were done graphically by passing one axis through the most predominant cluster and retaining the second orthogonal to it. The loadings so obtained are shown in Table 1. Inspection of Table 1 reveals that there is considerable similarity in the factorial patterns found in the several matrices. The summary presented in Table 2 reveals the patterns quite clearly. Factor 1 seems to be defined in terms of *thoughtful, conscientious, intelligent, persistent, and emotionally stable*. Factor 2 is defined by *cyclothymic and friendly*. The terms *dominant, co-operative, and sympathetic* have substantial loadings on both factors.

Table 1. *Rotated factor loadings from 14 correlation matrices*

Teacher Class	A 10a		B 8b		B 8c		C 7b		D 8b		E 7b		F 4d	
Trait	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂
riendly	03	93	30	65	14	76	24	88	67	12	80	35	03	83
elligent	65	31	33	48	66	46	58	46	83	26	53	63	77	43
otionally stable	42	-13	74	-11	72	-31	44	07	88	-12	52	01	63	13
ominant	36	55	35	75	-02	72	46	-08	12	68	-03	85	66	40
yclothymic	12	87	-05	61	-20	77	02	87	-01	73	10	78	-02	60
mpathetic	08	75	53	24	40	72	57	41	71	-09	80	-35	02	80
onscientious	75	-08	93	13	89	-06	80	34	91	15	59	-46	61	22
o-operative	20	61	66	40	47	68	67	21	89	-14	83	-05	06	78
ersistent	74	41	65	44	83	33	74	00	86	25	42	58	65	31
onventional	35	18	00	82	09	70	61	18	-04	-67	06	63	54	09
oughtful	90	-05	83	28	86	-01	72	-08	83	20	87	02	81	37

Teacher Class	G 4d		H 9a		I 9a		J 8a		K 10a		K 11a		L 10b	
Trait	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂	F ₁	F ₂
riendly	08	67	52	22	45	77	17	73	14	61	00	94	14	78
elligent	86	15	83	16	88	07	76	29	74	23	86	07	88	21
otionally stable	73	-34	70	-06	72	22	64	-09	40	13	40	02	49	-06
ominant	-13	85	04	68	63	11	-11	38	89	04	74	07	37	51
yclothymic	-15	77	-03	54	22	82	-06	68	16	77	-05	79	08	69
mpathetic	55	-09	63	-03	58	39	61	55	-32	66	03	71	23	64
onscientious	75	-18	78	45	87	-02	84	-11	67	-03	71	10	74	-04
o-operative	51	29	90	-04	81	21	56	49	24	66	42	70	-02	67
ersistent	25	58	79	15	91	-03	40	07	81	15	67	-15	44	11
onventional	-37	51	01	71	-08	42	52	11	72	-07	91	-05	61	31
oughtful	79	-38	72	40	88	-08	79	12	84	25	88	-02	80	-05

Table 2. *Summary of data on loadings from 14 correlation matrices*

Trait	No. loadings on F ₁ >0.60	No. loadings on F ₂ >0.60	Median F ₁ loading	Median F ₂ loading
Friendly	2	11	15	75
Intelligent	11	1	77	27
Emotionally stable	8	0	63	-06
Dominant	4	6	35	53
Cyclothymic	0	13	01	77
Sympathetic	4	6	54	48
Conscientious	13	0	77	-03
Co-operative	6	6	53	45
Persistent	10	0	71	20
Conventional	4	4	22	25
Thoughtful	14	0	83	01

IV. DISCUSSION

From our results it would appear that the circumstances under which our teachers would use the term *intelligent* have a great deal in common with those under which they use *thoughtful*, *conscientious*, *persistent*, and *emotionally stable*. The fact that ratings on these five trait names tend to vary together gives us some insight into the situations which control the use of *intelligent*. It also appears that judgements of *cyclothymic* and *friendly* have virtually no relationship to judgements of *intelligent*. The variables controlling our teachers' use of such terms as *friendly*, then, are not those controlling their use of *intelligent*.

It is interesting to note that the two factors which we have extracted in this analysis are similar to two of Cattell's factors. The factor which he refers to as 'general mental capacity' includes 'intelligent', 'conscientious', 'thoughtful', 'stable emotionally', 'intellectual', and 'wise'. Apparently this is very close to our Factor 1. The factor which

he refers to as 'cyclothymia' includes 'outgoing', 'good-natured', 'friendly', 'cheerful', and 'trustful'. This factor is quite similar to our Factor 2.

The similarities between the results of our analysis and that of Cattell would suggest that in Lebanon as in America the variables controlling the use of *intelligent* are frequently the same as those controlling the use of *conscientious*, *thoughtful*, and *emotionally stable*. There is, then, considerable overlap in the 'meaning' of these terms in both countries. No evidence was found for a unique use of *intelligent* on the part of bilingual teachers. Consequently, the low correlations between intelligence ratings assigned by Lebanese teachers and performance on 'culture-free' intelligence tests, Keehn & Prothro (1955) can be interpreted as reflecting unfavourably on the validity of those tests.

This investigation points up an important application of the factor analytic approach to 'meaning'. Osgood's suggestion that this technique is useful for studying cross-cultural problems in communication is borne out. Such analysis provides an approach to the perennial problem of whether 'meanings' are the same in investigations which involve the verbal behaviour of persons in different cultures.

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PUBLICATIONS RECENTLY RECEIVED

Body and Mind in Western Thought. By JOAN WYNN REEVES. Harmondsworth: Penguin. 1958. Pp. 403. 5s.

Psychologists, as Dr Reeves points out in her introduction, fall into two groups, those who 'put aside all questions about body and mind as philosophically senseless and scientifically a waste of time', and those who hold that they may provide a key 'to better understanding of psychological theory and practice'. Her book will certainly be welcomed by the latter group, and may well interest and even convert some of the former.

As Pelicans go this is a big book. It is, too, a finished book, admirable in its style, and obviously compiled with much loving care. It consists of two parts. The first is a long historical introduction of nearly two hundred pages by Dr Reeves herself; the second, a selection of extracts from Western thinkers ranging from Hippocrates and Plato to Wundt and William James. A set of eighteen plates of illustrative works of art (ironically enough sandwiched in among extracts from Plato who would have disapproved) contrast with the abstract arguments of the thinkers and remind the reader of the concrete variety of human nature and the vicissitudes of human culture. This indeed is Dr Reeve's striking merit and achievement. She has a keen sense of life and complexity and an awareness of the tides of historical change. She sees every stage of long debate on mind and body set against a background of conflicting cultural and social forces. She brilliantly conveys the emotional overtones which have made the whole topic much more than a mere matter of evidence and logic. The problems of body and mind 'grew so entangled', she points out 'partly because of the age-long identifications of matter with the sensory (and sensual), the emotional (and licentious), the enclosing (the soul's or devil's container), the inert (unresponsive), or the spatially relatable (soon to mean "the determined")... "Mind" by contrast was apt to connote the thinking (and pure), the rational (and controlling), the incarcerated (but immortal), the vital (and spontaneous), the unextended (therefore free)' (p. 74). These identifications coloured the thinking of the seventeenth, eighteenth and nineteenth centuries, and indeed are partly responsible for 'the chaos from which we have by no means emerged'. Again and again just because of the breadth and richness of her approach Dr Reeves provides the reader with illuminating insights which a 'bare bones' account of mind-body theories would have missed.

She is perhaps for that very reason less successful in delineating the main lines of the development of theories about minds and bodies—the gradual purification of animism from its first superstitious beginnings to its spiritual culmination in scholastic philosophy (though Dr Reeves documents the obverse demonology and quotes from the *Malleus Maleficarum*); the majestic and inexorable progress of the mechanical theory of the physical universe through its nineteenth-century zenith to its twentieth-century dénouement; the development of the biological sciences and their gradual expulsion of vitalism; the rise from Descartes onwards of the concept of consciousness, hardly known to the ancients but becoming in the early modern period the key concept of psychology and a crucial issue in philosophy; the overthrow of this concept from its central position as a result of new forces; and finally the contemporary confrontation of a physics quite undogmatic about the nature of 'matter' and a psychology equally undogmatic about the nature of 'mind'. There is in the whole story a drift and a trend; there is, I would think, a grand movement which transcends local circumstances. All this I find somewhat blurred in Dr Reeves's 'projection' by the very intricacy and subtlety of her detail. For that reason her book is more likely to be appreciated by the sophisticated reader rather than by the beginning student in psychology, for whom a clear-cut, even if over-simplified, framework is desirable.

About her own introduction Dr Reeves is engagingly modest. It is 'no more than a sketch map drawn to the editor's projection, to be thrown away, or at most employed as a lamp-shade'. Dr Hughlings Jackson when he bought a book which was too fat for his pockets used to tear off the covers and then split the book in two, much to the surprise of the bookseller in whose presence this act was performed. Few of Dr Reeves's readers are likely to do likewise with her Pelican; and if they did they would not throw her half away. No doubt there will be some who wish to modify her 'projection', for projections are subjective things. My 'projection' would give a larger place to eastern influences in western thought. From the time Alexander conquered the east, oriental religions and philosophies bulked larger and larger in the west, and the new civilization that arose from the ruins of the Roman Empire was an amalgam of

east and west. The west without Christianity and its eastern component would not be the west as we know it, nor would western thinkers have psychologized as they did. Neither Pythagoras at one end nor Jung at the other end of the long succession can be understood without the influence of eastern thought, and in so far as the orient became domesticated in the west it became an integral part of the western mind.

In modern times Dr Reeves omits both Marx and psychic research from her map. They both seem to me to be sufficiently important to include. Marx had perhaps as much as anyone to do with the rise of a biosocial view of mind and personality though his influence is not always acknowledged. In any case his contemporary dominion is so widespread that he ought not to be ignored. As for psychic research, it cannot be denied that it has coloured the views of at least some psychologists of the front rank. If its evidence is acceptable, the problems it raises are formidable.

Dr Reeves's anthology has been skilfully done and is just the right mixture of solid fare and idiosyncratic condiment to be really palatable. Obviously two hundred pages cannot contain everything, and it would be pointless to complain of omissions. All goes reasonably well until the nineteenth century. Then the Pelican loses its appetite and shows signs of satiety. Thirty-six pages from Braid, J. S. Mill, Hering, Wundt and William James is all it can manage and this is not doing justice to the fare provided by the nineteenth century. I can see no solution to this but to ask Dr Reeves to write another book on 'Body and Mind in nineteenth and twentieth century Western Thought'. Meanwhile she must be thanked for a book which will add a new dimension to the thinking of many present-day students of psychology. Perhaps the simultaneous publication of Heisenberg's *The Physicist's Conception of Nature* which does for physics just what Dr Reeves has done for psychology (combining a commentary with an historical anthology) will convince even tough-minded scientists on the necessity of her enterprise.

L. S. HEARNshaw

Educational Psychology and Children. By K. Lovell. London: University of London Press. 1958. Pp. 272. 18s.

A moderately priced text-book cannot afford to be lengthy. Dr Lovell's book succeeds in covering a wide field for its moderate length. The first nine chapters range over general psychology, including motivation, abilities, personality, perception, maturation and learning. The second nine chapters have particular educational relevance and cover educational guidance, backwardness and maladjustment, as well as a sketch of child development on the Gesell pattern. (Two concluding chapters, on physiology and statistics, are rather awkward appendages, being too condensed to do more than bewilder and dismay a student.) The author has performed a considerable task of abstraction to cover this range of topics without omitting contrasting points of view: thus, his nine-page chapter on 'Motivation' describes instinct theory, psychoanalysis, social theory, field theory and learning theory. Inevitably the text is indigestible at times. Only a much longer—and consequently more expensive—book could have provided this width of coverage with full detail and illustration.

There is plenty of detail. The book indeed is written rather in the style of the journal article, with footnotes identifying each contribution with author's name and reference. It is written on the basis of published research, with over 400 references in the text. Two-thirds of these references are to books or papers published in Britain (or jointly in Britain and U.S.); nearly half the references are dated 1950 or later; and over fifty refer to papers in post-war numbers of the *British Journal of Educational Psychology*. This emphasis on research will be welcomed by those who wish to depart from the tradition of writing a psychological text as literature. There are no anecdotes here, nor any illustrative John or Mary whose artificial case-history presses home a point. The presentation is clear enough, but dull: by Flesch's formulae, its Readability Index is 44 but its Human Interest score is only 8 (on a 100-point scale). Whether a text-book should be full of human interest is a debatable point, for the chatty introduction and the inspiring conclusion (to be found, for example, in Hughes and Hughes's *Learning and Teaching*) do not have the same appeal to the student working for his examination as to the general reader. However, an illustrative example may earn the valuable space it occupies by adding to the clarity and memorability of a difficult point; and though some examples are given here, in general the treatment is highly abstract.

Consequently, I doubt if this book will convert many teachers to a recognition of the value of psychology in education. It will give students for whom it is prescribed a clear idea of recent psychological research; and if they are not impressed by the relevance of what they read to the practical task of classroom teaching, it may not be so much the fault of the author as a reflexion of the bias of recent educational research in this country. Where the relevance of a chapter to education seems slight, the author has

included a section on practical points to note, but these do not always arise directly from the research which he has been describing. The author does not follow the common practice of other text-books in giving at the end of each chapter a list of practical exercises for the student, or of points to discuss, or problems in which to apply the principles discussed. Had he done so, he might more readily have escaped the accusation of writing a book designed to prepare students for a written examination, rather than to prepare them for their practical work of teaching. Nevertheless, among those who will find this book of value, not the least will be teachers of psychology, for whom it will provide a brief, competent and manageable review of recent work in both educational and general psychology.

JOHN NISBET

Psychotherapy of Chronic Schizophrenic Patients. Ed. by CARL WHITAKER. London: Churchill. 1958. Pp. ix+219. 35s.

A New Approach to Schizophrenia. By J. I. STEINFELD. London: Hutchinson. 1958. Pp. 159. 21s.

Psychotherapy of Chronic Schizophrenic Patients takes the form of an informal discussion among a small group of American psychiatrists who meet annually to exchange views on the diagnosis and treatment of schizophrenics. The book consists of eight chapters each comprising a discussion of some aspects of schizophrenia and led in turn by one of the eight members of the group. The current American preoccupation with communication in psychiatry, represented by the group's belief 'that people communicate more to one another than what they say', results in every word spoken in discussion being faithfully reproduced in print and, at times, this proves tiresome to the reader. As the group are all experienced psychotherapists, a large measure of agreement is reached on the most suitable psychotherapeutic approach towards schizophrenic patients. This consensus is, however, noticeably absent in the group's discussion of the diagnosis of the schizophrenic condition. Indeed it is admitted in the summary following the chapter on 'Diagnosis and Prognosis' that the foregoing discussion is 'somewhat confused and disorganised'. When, in succeeding chapters, the group pool their therapeutic experiences one is very impressed by their ability to understand, communicate with and apparently relieve schizophrenic patients. The general aetiological viewpoint expressed is that the schizophrenic patient is a resultant of a basic family disorder, and two of the chapters centre specifically on the treatment of the patient within the family group.

The only newcomer to the panel, invited specially for this conference, is the anthropologist Gregory Bateson, and it is his contribution on 'Schizophrenic Distortions of Communication' which provides the most stimulating discussion and goes nearest to producing a coherent theory of the schizophrenic process. Bateson presents an interesting account of his 'Double Bind' theory where the basic disorder of schizophrenia is seen to be a distortion in the classification of messages. His thesis is well presented and adds support to the views of Schilder, Cameron, Arieti and others who have also been interested in schizophrenia as a disorder of communication. Bateson also raises such worthwhile questions as whether one can validly speak of 'unconscious judgement' and whether we have any right to assume purposive behaviour behind the schizophrenic's process. Unfortunately, such questions are largely ignored due to the group's preoccupation with psychotherapy which fundamentally rests on the assumption that schizophrenic behaviour is purposeful to the patient. The book is undoubtedly well worth reading by anyone interested in the fascinating problems raised by the schizophrenic patient, particularly those with a therapeutic orientation.

While Whitaker and his group consider that the schizophrenic patient has been starved of affection within the family group, Steinfeld in *A New Approach to Schizophrenia* suggests that the schizophrenic has been literally starved. In his opinion, all schizophrenics suffer a severe early hunger trauma during the first few weeks of life at a time when the infant has a purely vegetative existence. As a result of this trauma the individual is conditioned to abnormal vegetative reactions which inhibit the normal supremacy of cortical functions in later development. The author's rather startling approach to treating the schizophrenic is simply to reproduce the original hunger trauma directly by starving the patient, or indirectly to recapitulate the original state of infantile acidosis by having the patient inhale acetone while under an anaesthetic produced by E.C.T. or Pentathol. Steinfeld reports remarkable improvements in patients who have been treated in such a way, but is careful to add that, in his view, there is no cure for schizophrenia, the therapeutic aim being limited to increasing the periods of remission. He suggests that we must turn our thoughts to the prophylactic measures which can be taken to ensure that the predisposing hunger trauma does not occur. In this direction, he recommends a nation-wide campaign to encourage

unlimited feeding on demand to ensure adequate early nourishment. The book contains thirty-nine illustrative case histories of schizophrenic patients who all demonstrate the typical vegetative history which our author believes may be found only in schizophrenics. In almost half the total group of 134 patients stated by the author, early histories were unobtainable, but it is nevertheless assumed that the missing cases 'would probably have shown the specific early history had we been in a position to study it'.

The author closes with a list of the elements which constitute a typical vegetative history and perhaps the chief merit of the book is that the hypothesis presented is accessible to objective validation. One feels doubtful, however, considering the apparently world-wide incidence of schizophrenia, that such a specific and simple explanation of this condition is likely to be valid.

ANDREW MCGHIE

Attempted Suicide: its Social Significance and Effects. By E. STENGEL and NANCY G. COOK. Maudsley Monographs No. 4. London: Chapman and Hall. 1958. Pp. 136. 25s.

The range of suicidal behaviour extends from suicidal threats and gestures at one pole to the completed act at the other. Previous studies of attempted suicide have been made chiefly with the intention of contributing to the knowledge of suicide. Professor Stengel and his colleagues have broken new ground by studying the problems peculiar to attempted suicide itself, in addition to those it has in common with suicide. In their definition of a suicidal attempt they included slight injuries with no danger to life, provided that these appeared to be consciously aimed at self-destruction: and in an investigation of more than 600 cases admitted to London hospitals develop their thesis that the suicidal attempt should be viewed as a social behaviour pattern in its own right. A parallel study of 117 cases of suicide indicated that the social element in suicide, though not absent, is much less manifest.

Much evidence is adduced to justify the focusing of attention on the life-preserving tendencies inherent in suicidal attempts which are considered by the authors to be no less significant than the aggressive-destructive trends. They point out that a comparatively small proportion of those who have attempted suicide finally kill themselves, and that only a minority of suicides have attempted suicide before. They claim that most suicidal attempts seem to take place within the individual's social field and in such a way that the intervention of others seems possible or even probable. The outcome depends therefore not only on the strength of the individual destructive drives but also on the response of the human environment to this social crisis.

Gestures and threats of suicide are well-recognized and common ways of manipulating the environment to the individual's own advantage. In most if not all suicidal attempts Stengel and his colleagues identify a similar social orientation, an 'appeal element' or call for help. The suicidal individual is usually unaware of this appeal element which makes it all the more potent in eliciting the guilt feelings which so often characterize society's response.

Stengel and his colleagues also describe an 'ordeal character' of the suicidal attempt, a testing or proving of the balance of libidinal and destructive impulses in the self the outcome of which the subject appears, usually fatalistically, to accept.

The variety of social effects of suicidal attempts, to which few previous writers have given attention, are described and emphasized as being as significant as the social factors whose etiological importance is widely accepted. The resulting modifications of human relationships, the reactions of mourning and reparation in relatives, and the punitive social reactions expressed in the present law of England on attempted suicide, are illustrated and discussed. Few would disagree that in assessing the suicidal risk and in organizing appropriate treatment the reactions of the social group must be taken into consideration, and that discovery of particular social factors which are important in causation, for example, isolation, has important implications for the prevention of suicidal acts.

It is probable that the appeal element in suicidal acts is not so constantly present as Stengel and his colleagues claim: it is, for example, at the least much less conspicuous in the suicidal attempts of old people than in those of young hysterics. The authors may also go too far in their suggestion that those who have attempted suicide and those who have committed it constitute two different though overlapping populations: they themselves point out that the incidence of suicide in those who have attempted suicide is high compared with the incidence of suicide among the general population. These doubts and the new vistas opened by the authors in their elucidation of the social setting should stimulate further research. The major hypotheses are clearly stated and may be tested. Throughout, the monograph is lucidly and persuasively written, and alike in its careful factual reporting and in its original theoretical formulations is an important contribution both to psychiatry and to sociology.

I. R. C. BATCHELOR

The Neuroses and Their Treatment. Ed. by E. PODOLSKY. New York: Philosophical Library. No date given. Pp. xiv + 555. \$10.00.

This collection of thirty-seven papers is intended to provide a comprehensive survey of contemporary (American) theory and practice concerning the diagnosis and treatment of neurotic conditions. Consistent with this aim, the patients described range from young infants to the aged and their disorders from relatively mild emotional reactions to everyday stresses to severe and complex neurotic and psychosomatic conditions. The methods of therapy described include varieties of psychotherapy, psycho-pharmacological procedures, physical treatments and psycho-surgery.

The intended reader is less well defined. Psychiatrists and clinical psychologists are likely to consider the discussions insufficiently detailed and will certainly regret the complete lack of bibliographical references and an index. Companion volumes in this series, on literary, philosophical, artistic and general scientific topics appear to be aimed at the 'intelligent layman', but he would experience considerable difficulty with the technical vocabulary of the present volume. Possibly the work is best suited to the needs of physicians in general practice for whom several of the papers appear to have been specifically written.

The papers constituting the chapters were culled from publications in psychiatric journals by 'eminent authorities in the field on all phases of the neurosis'. Their very eminence dictates the nature of the treatment which is mainly that of the 'authoritative clinical report' with its attendant weaknesses. In most chapters a theoretical viewpoint or interpretation is discursively expressed and illustrated by reference to selected clinical histories described as typical. No controlled experimentation is reported even when the title of a chapter refers to experimental studies. Similarly no statistics of greater sophistication than a percentage are presented except in one chapter by Miles, Barrabee and Finesinger on the evaluation of psychotherapy. Following a review of the literature, these authors make a thorough attempt to assess the effects of psychotherapy on sixty-two cases of anxiety neurosis treated in a general hospital.

Many chapters are devoted to discussions of aetiology and most of these assume the general validity of psycho-analytic theory though, frequently, minor or major specific modifications or additional hypotheses are proposed. Similarly, almost all discussions of psychotherapy assume the efficacy of the psycho-analytic techniques of free association and manipulation of the 'transference' situation, though theoretical explanations of these effects vary. This is even true of one chapter by Zuger who proposes, on the basis of a somewhat metaphysical theory, that the aim of therapy is to make the conscious unconscious.

Several of the chapters devoted to therapy are concerned with the use of 'tranquillizer' drugs. These tend to display an over-optimistic attitude and lack of objective evaluation of therapeutic value. Bayart, in an extreme example, states: 'The person treated with hydroxyzine retains full consciousness of the stimuli, affective or otherwise, that released this disturbance. He reports their nature and their intensity but his reactions have become those of a completely balanced individual.' Concerned with psycho-pharmacological treatment of a different type is Meduna's description of his development of carbon dioxide therapy, a fine example of the 'tough-minded' approach to clinical research.

The main interest in this volume, at least for the reviewer, lies in the many excellent descriptions of case-histories illustrating the wide range of symptomatology and behaviour which may be described as neurotic. As always, there is also much to interest a psychologist in the speculations of experienced clinicians and when these are based on the careful observations of, for example, a Spitz, he is stimulated to reassess the aptness of his own theoretical frame of reference to the data described.

H. GWYNNE JONES

The Testing of Negro Intelligence. By AUDREY M. SHUEY. Lynchburg, Virginia: J. P. Bell. 1958. Pp. xv + 351. No price given.

As the author herself says, this book might properly be called *A Comparative Study of the Intelligence of American Negroes and Whites*. Her address, Lynchburg, Virginia, and her acknowledged indebtedness to her students in the 'Racial Psychology Classes' (at Randolph-Macon College) are sufficient indicators of her motivation and purpose in writing a book on this subject of lively debate in the United States.

The book is a painstaking and careful review of approximately 240 experimental studies, over 44 years, of negro intelligence, of which all but two were carried out in the States. The studies chosen deal primarily with intelligence as measured by *standard* psychometric tests, some sixty of which are involved. Dr Shuey

groups these studies into eight primary divisions to each of which she devotes a chapter. After an introductory chapter, she continues with one on Young Children, that is, children primarily in nursery and kindergarten schools. There follows chapters on School Children, High School and College Students, the Armed Forces, Deviates, Delinquents and Criminals, and Racial Hybrids; next comes a chapter on Selective Migration; then a final chapter summarizing the findings and giving the author's conclusions.

The results of the various studies in the several chapters are conveniently presented in tables giving the author, date and place of each study, the number of subjects involved, their ages, the method by which they were selected and the results. The latter are clearly distinguished from the comments and conclusions of the investigators thus enabling the reader to assess the data without prejudice. When white subjects are included in the study data comparable to that given for negroes are tabulated separately.

Having assembled the data, Dr Shuey attempts an evaluation of them. The conclusions to be reached from this evaluation are inescapable. Everywhere, and with remarkable consistency, the results indicate a discrepancy between the average intelligence of negroes and of whites in favour of the latter, irrespective of whether the comparison is between actual groups of negroes and whites tested, or between negroes and general population norms. Admittedly the difference is less emphasized amongst the young children, but here it is pointed out that selection factors are involved. It is suggested too that the negroes are often more handicapped by poor environment as they grow older than are whites, while an overall negro deficiency in vocabulary is underlined. If we protest and ask 'But what of the differences between northern and southern states?' we find that this has already been thought of and ample evidence is provided; to take but one example, amongst the Armed Forces in the First World War intelligence ratings fell off in the order northern whites, southern whites, northern negroes, southern negroes. Or again, if we look at Deviates, Dr Shuey estimates that amongst the gifted children there were only one-sixth to one-third as many negroes as whites, whilst amongst retarded children the negroes outnumber the whites by 6 to 1. Social and cultural factors too have been taken into consideration, so the case seems proved.

Now there is no reason why negroes should not differ from whites in intelligence. They differ from whites in pigmentation, in bone structure, in distribution of blood groups, and no doubt in other ways. But where intelligence is concerned we must be careful, not only because of the slur likely to be cast since high intellectual potential is at a premium in the world today, but also because intelligence, since it can only be measured indirectly, is difficult to measure accurately.

To compare negroes and whites impartially where intellectual ability is concerned would require the construction of special batteries of tests for which an item analysis would require to be carried out separately on equal stratified samples of negroes and whites and the items subsequently chosen so as to give both races an equal chance of success. Some of the investigators whose work is reported were aware of this; Young and Bright, for example, in their 1954 study, question the suitability of the W.I.S.C. for southern rural negro children. Although Dr Shuey reports this and draws attention to a few investigators who noted some test-item differences between the racial groups, she does not lay sufficient emphasis anywhere on the possible limitations of the tests employed. Possible bias on the part of testers is another factor which, in a well-planned investigation, would require to be taken into account: a careful examination of the data which Dr Shuey presents suggests that bias may well exist. However, these are points to be kept in mind when evaluating future studies: for the present, we must congratulate Dr Shuey on the interesting review she has given us of the literature to date.

A. E. MAXWELL

The Individual Psychology of Alfred Adler: a Systematic Presentation in Selections from his Writings. Ed. by HEINZ L. ANSBACHER and ROWENA R. ANSBACHER. London: Allen and Unwin. 1958. Pp. xxiii + 503. Price 30s.

Adler's numerous books consist largely of written-up lectures and republished articles. They are full of valuable material, but they are diffuse and repetitive, and none of them contains a systematic exposition of Individual Psychology. He is thus a particularly suitable subject for the sort of treatment he has received in the book under review. It consists of extracts from his writings, grouped into sections and subsections, interspersed with editorial comment, and prefaced by a short introductory essay. The editors, Professor Heinz and Dr Rowena Ansbacher, have both had extensive personal contact with Adler, and have a strongly sympathetic, though not uncritical, attitude towards Individual Psychology, which they describe as 'a socially oriented, subjectivistic, holistic psychology of attitudes', as contrasted with Freud's 'biologically oriented, elementaristic, objective drive-psychology'.

The editors have done a remarkable job with scissors and paste. It is not until one has studied the bibliographical note on p. xiv, and mastered the detailed but unobtrusive system of source-referencing, that one realizes that the selections from Adler's writings consist, for the most part, of mosaics of short extracts from different sources, so skilfully put together that the joins hardly show. For example: the chapter on 'Understanding and Treating the Problem Child' begins with a subsection, occupying just over a page, entitled 'The Dynamics of Problem Behaviour'. This contains five paragraphs, drawn from four different sources—two from *Social Interest*, one from *Understanding Human Nature*, and two from previously untranslated articles. Adler, incidentally, has been badly served in the past by his translators, and the present editors have often preferred to translate afresh, even when quoting from books, like the two above-mentioned, of which standard English versions exist.

It would be a pity if anyone interested in Individual Psychology were to confine himself entirely to the book under review, as something of Adler's flavour has evaporated in the rigorous boiling-down process to which he has been subjected. But for sheer nutritive content, no single book by Adler can compare with the present selections. Students of Individual Psychology—and still more, perhaps, those who lecture on it—have good reason to be grateful to the editors.

MARGARET KNIGHT

Function Fluctuation. By C. C. ANDERSON. *British Journal of Psychology Monograph Supplement No. 30.* London: Cambridge University Press. 1958. Pp. vii + 104. 21s.

This monograph takes up an important problem in mental testing on which there is a surprising amount of speculation but relatively little evidence. As is well known, the fact that two tests of one and the same function fail to correlate perfectly is due partly to errors associated with the test, and partly to 'function fluctuation', which is a term originally suggested by Thouless to denote a day-to-day quantitative variation in the function tested. The first half of this monograph reviews the literature on this and related concepts and tries to clarify the undoubted confusion of terms and concepts existing in this field. Also included in the first part is a chapter giving results of previous research. The review is clear and to the point, although at times it appears a little niggling and ungenerous to some of the earlier workers. There is also a superabundance of quotations more characteristic of a Ph.D. thesis than a monograph; these do not always contribute greatly to the argument. However, taken by and large, the reader of this section should have a fairly clear grasp of the problems and theories in this field. Not all relevant material is included; thus, no mention is made of the work of Fleischman, or of the Hartshorn and May studies. The second part of the monograph deals with the author's own experiments which appear to have been unusually extensive as far as the number of tests administered is concerned. Analysis, presentation and discussion are clearcut and incisive. The main results are that function fluctuation is active both in cognitive and in non-cognitive functions, but more extensively in the latter. Function fluctuation does not appear to be a characteristic feature of a given individual; correlations between function fluctuation on one test and function fluctuation on another appear to be nearly zero. From these results the author derives a number of conclusions which are sensible, and a theory of cognitive function fluctuation which does not seem a great advance to the reviewer and which, if true, would suggest that correlations of function fluctuation should be obtained between different tests. However that may be, we are in debt to the author for both his review and his experimental contribution to an important aspect of test psychology that has been somewhat neglected in the past.

H. J. EYSENCK

Electronic Instrumentation for the Behavioral Sciences. By CLINTON C. BROWN and RAYFORD T. SAUCER. Springfield, Illinois: Charles C. Thomas. Oxford: Blackwell. Pp. xiv + 160. 42s.

There is a growing need for a work which achieves what this book promises, a simple account of electronic theory, valves, transistors, circuitry and measuring instruments for research workers who have no previous knowledge of electronics. The need remains, and to fill it demands a degree of competence and clarity not displayed here. A few samples of the book's faults are as follows. Factual errors—R.M.S. value is about 70% of peak to peak value; frequency is number of reversals per unit time: a sine wave has a uniform rate of change. Confusion of symbols—in a single diagram may be found MMF for $\mu\mu\text{F}$, MH for mH, MA for mA, M for M Ω , K for k Ω , 8 for 8 μF , 100 for 100 $\mu\mu\text{F}$, 6.3 for 6.3 V, 47 for 47 Ω (these interpretations and corrections should not be left to a beginner), elsewhere mf, mfd or MFD for μF . Equations with undefined terms, terms wrongly identified in the text, and no units. Use of advanced concepts with little or no explanation—transients, decibels, phase shift, hard radiation, work function,

thermal noise (who is familiar with these yet needs a simple account of Ohm's law or reactance?). Solecisms—charge equal and opposite to impressed EMF; R_1 , R_c , etc. referred to in the text but not marked in the circuit diagram; incorrect explanations of grid leak bias and ohmmeter. There are also contradictions, unclear deductions, false impressions, grammatical errors, misspellings, misprints, faulty diagrams, text references to the wrong figures, and at least one wrong reference in the brief index.

The potential reader can get an accurate, lucid, non-mathematical account of much of the contents, at a third of the price, in *Foundations of Wireless*, by M. G. Scroggie (Iliffe).

J. R. TROTTER

Personality and Motivation Structure and Measurement. By RAYMOND B. CATTELL.
London: Harrap. 1958. Pp. xxiv+948. 55s.

This is a whale of a book in more ways than one. Its weight and size are impressive; for his 55s. the reader gets 948 pages of text, including 661 references of which exactly 100 refer to Cattell's own work. The book weighs 2 lb. $1\frac{1}{2}$ oz., which works out at 1 shilling an ounce, roughly. Again, just as the whale, although living in the sea, is not a fish but a mammal, so Cattell, although writing on personality and motivation, is not at all typical of the modern clinical psychologist. The stress in the title lies on the words 'Structure' and 'Measurement', and for the determination of the former and the accomplishment of the latter Cattell relies very much on complex statistical methods, in particular oblique factor analysis (i.e. a method of factor analysis in which factors need not be independent of each other, but may themselves be correlated) and the discovery of higher-order factors (i.e. factors derived from the inter-correlations of the original oblique factors).

The book is, in a way, a continuation of Cattell's previous volume entitled *Description and Measurement of Personality*; as Cattell says 'For the exact definition of such concepts as ability, interest, attitude and temperament... and for examination of evidence on factors for which only final patterns are given in this book, the researcher should consider *Description and Measurement of Personality* as Volume I of the present book'. This dependence on former publications is characteristic of the book throughout; I doubt if readers not familiar with a good deal of Cattell's previous writing could follow the argument at all well. This feature is a little unfortunate, particularly in a book of such length. It will certainly work against the author's hope that it will be useful for undergraduate instruction. Another reason why it may be doubted whether any but the most exceptional undergraduates can be recommended to read this book is related to Cattell's use of factor analytic techniques. While he does attempt in the introductory chapters to introduce these techniques at an elementary level, he soon soars into the highest layers of complexity and argumentation where anyone but the most expert factor analyst would be left gasping for breath. It is not argued that such discussions should not be given, but the attempt to present them in the same book as simple introductions to correlational analysis for the quite uninitiated reader, causes a certain imbalance and an unnecessary lengthening of the book which could have been avoided.

Apart from such relatively trivial matters, what are the reader's first impressions of the book likely to be? Almost certainly he will be impressed by the tremendous enthusiasm and surgency running through the whole research enterprise of which this is a record, and he will admire the quite unusual fertility of ideas which is apparent on almost every page. No one interested in the field could read the discussion beginning on page 436 and culminating in Cattell's 'List of Principles for measuring manifestation of Motivation' without feeling that he is here encountering a writer extremely well read in the psychological literature bearing on personality measurement, and capable of generating novel and original ideas with uncommon facility. Cattell seems almost incapable of touching any subject without changing one's way of looking at it or without throwing up ideas, suggestions and research plans deserving of the most serious consideration. To the expert, therefore, this book will certainly be required reading, and no one working in this field can afford to isolate himself from the invigorating breath of air from Illinois.

There is, however, another side to this medal, and indeed the existence of this other side might have been predicted in terms of Cattell's own factors. The person high in surgency according to Cattell is energetic, original and assertive, but he is also hasty and lacking in self-criticism. The second impression which the reader is likely to carry away from perusing this book is that the ideas generated by Cattell, and the researches carried out by him, are very uneven in value. Some, particularly those relating to personality source-trait structure, are almost certainly valuable additions to knowledge and cannot be neglected by theorists or practical workers in this field. Others, particularly those dealing with dynamic structure, seem to the reviewer of relatively little value or interest. Indeed, on occasion the reader may wonder whether Cattell is quite serious in what he says. He claims, for instance, that drive structures can only be determined by factorial methods, and criticizes the usual work in this field done by experi-

mental psychologists. These criticisms are not entirely without value, but Cattell's own derivations of such concepts as the 'sex erg' do not impress the reviewer as being in any way superior. Essentially the 'sex erg' is defined by a pattern of intercorrelations found among certain attitudes such as 'I want to fall in love with a beautiful woman', 'I want to satisfy my sexual needs'; 'I like to see a good movie now and then'; 'I want to listen to music'; 'I want to travel and explore the world'. The reader must form his own opinion as to whether either method or results can seriously be said to mark an improvement on the work done by learning theorists in isolating and defining drives. One must agree with Cattell that drives can only be defined in terms of some kind of specification equation, but few people, I think, would agree with him that this equation should be a factor analytic one rather than, say, of the type introduced by Hull.

The same feeling of a lack of critical evaluation strikes the reader in considering Cattell's views about the different methods of factor analysis as demonstrated in his 'co-variation chart' on page 494. Correlations between tests, and their analysis by some form of factorial method, are almost certainly useful and valuable adjuncts to more strictly experimental work. Whether correlations between persons or between occasions, or any of the other combinations suggested by Cattell, can be of much value, except in isolated cases, remains to be proven. Many good critics are sceptical, and Cattell's enthusiasm for these methods, interesting as they are on the technological level, hardly seems justified by the results achieved.

Cattell's book, therefore, must be read critically; the reader must never forget that the ideas he meets are thrown off like a shower of sparks by a catherine wheel. There is an insufficiency of insulating material to prevent the poor and mistaken ideas from coming to the surface and thus dominating the good and valuable ones. As with many 'surgent' people, the 'censor' has been nodding. This makes an unbiased review of Cattell's book very difficult and I am sure that many readers will fail to appreciate its outstanding contributions by concentrating their critical interests on the more doubtful ones.

What then is Cattell's main contention? He claims to have isolated some sixteen main factors in the fields of what he calls *L*-data and *Q*-data, by which he means life record variables, such as behaviour ratings, and questionnaire data. He claims that the factors found in one of these fields correspond well with those found in the other on the whole. He also claims the discovery of a number of factors from the intercorrelations of *T*-data, by which he means objective test variables other than questionnaires. These factors do not agree well with those in the *L* and *T* fields. Cattell names his factors, sometimes by using psychiatric terms such as cyclothymia, and sometimes by coining new terms which are liable to cause a headache to most readers. Among these newcomers to the psychological scene are such things as parmia (para-sympathetic immunity), threctia (threat reactivity), prensia, coasthenia, comention and protension, antia, corticalertia and many others. A special set of questionnaires has been published to measure these variables, although unfortunately little seems to be known about their correlations with the more orthodox Guilford factors or many of the other currently used clinical tests.

Most of these factors had already been presented by Cattell previously. He now, for the first time, takes seriously the duty of anyone working with oblique factors to calculate second-order factors, and in doing so he arrived at a conclusion which, to the reviewer, appears a very happy and satisfactory one, namely that 'the publication of extensive analyses... yields two very striking findings—the general integration factor and the extraversion/introversion factor'. Critics have sometimes pointed to the apparent differences in factor structure between Cattell's sixteen factors and the reviewer's set of just two, to wit, neuroticism and extraversion/introversion, as evidence of the subjectivity of factorial methods. In *The Structure of Human Personality* it was argued at great length that extraversion/introversion and neuroticism were conceived of as second-order factors, whereas Cattell's work dealt entirely with oblique first-order factors. It was also suggested then that if the appropriate analysis were done, there was no reason to anticipate any disagreement between the two sets of results. Cattell's finding of extraversion/introversion and integration or adjustment as second-order factors from his own work, suggests that this hypothesis was correct, and it would seem to follow that factorial procedures are perhaps more reliable and valid than some critics have claimed. It may be of interest to note here that, in order to check the identification of Cattell's and his own factors, the reviewer carried out a separate experiment for the purposes of this note, correlating Cattell's measures of these two second-order factors and his own; the resulting correlations were positive, high and highly significant.

Even in an extended review there is not enough space to enter into any further discussion or criticism of Cattell's work. A few points may be worth mentioning, however, in concluding this review. In the first place, the reader will find many examples of Cattell's exceptional verbal facility and his often somewhat biting wit. To quote just one brief example, 'Dexterity in handling a crossbow, or an interest in Guatemalan centipedes, contains specifics little used in High School counselling'. This mordant wit is used with great effect in his discussion of modern clinical, educational and industrial practices; with most

of his criticisms here it would be difficult to disagree, and one can only hope that the recipients of these guided missiles will be in place to receive them.

In the second place, Cattell is essentially a 'one-woman man'. He has remained faithful to factor analysis from his very first writings, and he disdains the attractions of other methodologies and theoretical systems based on them. Thus several times he goes out of his way to criticize modern learning theory and experimental psychology in general in a manner which is not likely to bring about that very essential *rapprochement* between experimental and psychometric methods so much needed and so strongly and well argued for by Cronbach in his recent presidential address to the A.P.A. The reviewer feels certain that Cattell is mistaken in these diatribes and that factor analysis, far from being an adequate method for solving all or most of psychology's problems, is only one method among many with which the psychologist should be familiar.

In the third place, there are certain technical points with regard to which one may feel a certain amount of disquiet. Cattell has always extracted a rather large number of factors from matrices of intercorrelations based on relatively small numbers of subjects. In view of the smallness of the original correlations, one is often doubtful about the significance of any but the first few factors. In his use of criteria for deciding when to stop the extraction of further factors, Cattell has always shown a preference for the least stringent criteria; such a choice may be thought to be mistaken. Cattell criticizes other writers for extracting too few factors, thus giving themselves too little space for rotation; it may be that Cattell extracts too many factors and allows himself too much elbow room in this respect.

The fourth point is related to this. Once having extracted his many factors, Cattell's procedure for identifying factors extracted from different matrices is again rather lenient and may leave the reader unconvinced. It might be interesting to introduce the new methods put forward by Ahmavaara into Cattell's realm of factors to see to what extent his claims are substantiated; unfortunately, his method does not appear to have been used by Cattell.

In his use of experimental methods for the measurement of personality, i.e. in the field of *T*-data, Cattell again shows much ingenuity in adopting techniques of flicker fusion measurement, dark adaptation, and so forth, to the assessment of individual differences. It is often difficult, however, to follow his interpretation of these measurements, because they tend to be of a rather rough and ready kind. There seems to be little control over what may be very important factors, such as pupil size, and the measurement itself is often heavily curtailed in order to make possible the administration of a large number of tests. This curtailment almost certainly introduces uncontrolled variables which may seriously interfere with proper measurement and determination of such variables as dark adaptation.

This leads me to the last of these points. Cattell might reply that as long as the experimental situation is identical for everybody, it fulfils the demands of the usual definition of a standard personality test. This, up to a point, is true, but it leaves out of account the equally important question of how results obtained in this manner are to be explained in terms of psychological theory. Cattell seems to be disinclined to take this task seriously; he sometimes appears to think that there is little psychological theory in terms of which such results can be explained. However, the very heterogeneous groups of tests which delineate his *T*-factors, and their failure to show much agreement with his *L*- and *Q*-factors may make him pause and reconsider this question. To find that poor dark vision correlates with neuroticism is interesting, but it is only a beginning; as Thurstone pointed out 'the coefficient of correlation is a confession of ignorance'. It tells us that there is some kind of causal relationship in our data, but it does not tell us the precise nature of this relationship. Is there a physiological inferiority involved? Does the anxiety produced by the testing situation interfere with the solution of the problem set by the test in the manner suggested by Sarason? Is there a direct conditioned fear reaction induced by being in the dark which affects the pupil through its autonomic connexions? Is there a failure of the more neurotic person to keep his eyes fixed to the fixation point? These are only some of the many possible hypotheses, and until an answer is found to the question posed by them, it would be dangerous to use measures of dark vision as personality tests. Conversely, could we but find an answer to these questions, it should be possible to increase the reliability and validity of these measures well beyond their present rather unsatisfactory status. Perhaps it is asking too much of one department to concern itself with these problems in addition to those of the more factorial kind; but my own feeling would be that to go on discovering new factors without having an adequate theoretical basis, firmly grounded in experimental psychology, for the existing one, is a policy of putting the cart before the horse. However, on this point no doubt much could be said from both sides, and it is probably unprofitable to go any more deeply into this question.

In summary then, as I said at the beginning, this is a whale of a book—fascinating, having a rather strange and unusual taste, and on the whole perhaps a little indigestible—nevertheless, something which excites admiration in many of its aspects.

H. J. EYSENCK